The Journal

OF THE

Royal United Service Institution.

Vol. XI. | 1867. | No. XLIV.

Evening Meeting.

Monday, March 4th, 1867.

Rear-Admiral Sir F. W. E. NICOLSON, Bart., C.B., in the Chair.

NAMES OF MEMBERS who joined the Institution between February 4th and March 4th, 1867.

ANNUAL.

Cooper, Egbert Wm., Lieut. 2nd W. I. Regiment. 1*l.*
Eyre, Henry, Major-General, Col. 59th Regiment. 1*l.*
O'Hea, John Bryan, Captain late 25th K. O. Borderers. 1*l.*
Hodge, De Burgho, Captain retired, 12th Royal Lancers. 1*l.*
Gillett, Henry, Capt. 13th Light Infantry. 1*l.*
Parr, Henry Hallam, Ensign 13th Light Infantry. 1*l.*
Gordon, J. J. H., Major-Commandant 29th Punjab Infantry. 1*l.*
Dillon, Martin, Lieut.-Col. 2nd Batt. Rifle Brigade. 1*l.*
Hutton, Thos. Bruce, Major 1st W. I. Regiment. 1*l.*
Linton, J. K., Master retired Indian Navy. 1*l.*
Brabazon, John Palmer, Capt. Grenadier Guards. 1*l.*
Alexander, J. H. I., Capt. R.N. 1*l.*
Seymour, Hugh de Grey, Capt. Grenadier Guards. 1*l.*
Tracy, Hon. Chas. Hanbury, M.P., Lieut. late R.N. 1*l.*
Black, Bladen West, Major-Gen. late R.A. 1*l.*
Cooke, Thos. Chas., Lieut. R.A. 1*l.*
Macdonald, Alex. J. J., Maj. Unatt. 1*l.*

THE ATLANTIC TELEGRAPH CABLES OF 1857-8, ALSO OF 1865-6.

By Staff-Commander H. A. Moriarty, R.N., C.B.

(Read by the Secretary.)

Upon being requested to contribute a paper upon telegraph cables, I felt as much at a loss how or where to begin, as if I had been asked

to write a treatise on a ship, not knowing whether to commence with her construction from the keel upwards, or to take her at once, fully equipped, into a gale of wind and a raging sea. In point of qualification also the analogy holds good, for although I have lived 35 years in ships, I do not know how to build one; neither am I a civil engineer or electrician; nor is it anticipated that this paper will be heard or read by an electrician, or civil engineer who has been engaged in telegraphy, as it cannot be expected that either will derive any information therefrom. My aim is merely to give to members of the sister services an outline of what has been done.

The progress of telegraphy since the first practical application of the influence which a galvanic current exerts when passing a magnetic needle, has been very rapid. Messrs. Cooke & Wheatstone arranged an electric telegraph in 1837. The first established successfully, and worked for a useful purpose in this country, was that on the Blackwall Railway, in 1842. About that time a proposition was made to the Admiralty for running a wire to Portsmouth, but so little was the new discovery relied on, that their Lordships decided upon having no other telegraph than the chain of semaphores then established. Nor can it be much wondered at, for between the peace of 1815 and that time, the unemployed officers of the Navy were as much occupied in discovering new telegraphs as other mechanical geniuses had been in discovering perpetual motion. As the Blackwall Railway was worked by an endless rope, the use of the telegraph was imperatively necessary. Even the ordinary railways, with the double line of rails, could not be worked safely without some such rapid mode of communication. Thus the railways became the foster-fathers of the electric telegraphs, but principally for their own business. As it was impossible to establish telegraphic communication across either of the channels without an effectual insulator, Dr. Montgomerie came to its aid in 1843, by publishing his valuable discoveries connected with the properties and uses of gutta-percha. It was not till 1851 that the bold project was carried into effect of laying a stout and strong cable from Dover to Calais, 23½ nautical miles in length, containing four separate conductors in one core; that line has given very little trouble since it was laid. Though two years elapsed before another attempt was made—in that from Dover to Ostend, 70 nautical miles in length—no less than 55 submarine cables have now been successfully laid by one company alone, not including the Atlantic cables. The cores of these 57 cables were made at the gutta-percha works in the City Road; a few more have been made by other companies. Of the 55 cables above mentioned, two only exceed 1,000 miles, that from Malta to Alexandria (1,351 nautical miles), and that down the Persian Gulf (1,276 nautical miles); both have the distance broken by being landed at intermediate stations. As the former cable has given much trouble, in consequence of passing over reefs in the comparatively shallow water of 40 fathoms or less, I believe it will be remedied either by replacing it by degrees with stronger cable, or by giving up the African stations and carrying a direct line, in deep water, from Malta to Alexandria.

Before passing from the comparatively small distances to the once

great problem of connecting the two hemispheres, it may be as well to premise that the experience acquired by these scientific men who had the responsibility of laying and maintaining those cables, induced them to believe that a very strong cable was necessary, while the opinions of some respected philosophers was against the attempt to cross the broad ocean, as they considered that the electric current could be of no practical utility beyond limited distances. Professor Sir W. Thomson, Sir Charles Bright, Mr. Whitehouse, Professor Wheatstone, Mr. Varley, Mr. Latimer Clark, and others firmly maintained the opinion which has been proved to be correct.

After the successful working of 12 or 14 minor lines, including the first (Dover to Calais), for five years only, and the longest (Italy to Corsica), of only 95·6 nautical miles, some bold projectors conceived the idea of forming a company to connect Europe with America; and the Atlantic Telegraph Company was established in 1856, or within five years of the effectual connection between England and France. Mr. Brett, Mr. Cyrus Field, Sir Charles Bright, and Mr. Whitehouse were the prime movers in what appeared to the majority of our countrymen as a wild and impracticable scheme. However, the company was formed, and £350,000 raised from among a few wealthy enterprising men, the shares being £1,000 each. The Governments of Great Britain and America encouraged the attempt by each offering a large annual subsidy as long as communication was maintained, while the North American provinces and the United States granted concessions to Mr. Cyrus Field, assuring him the sole right of laying telegraphic cables from any part of the United Kingdom to any of the opposite coasts, till south of the State of Maine. France also joined in this protective licence, which was to have terminated in March 1867, if telegraphic communication were not previously established. The British and American Governments also lent ships deemed suitable to carry the first cable, and for the purpose of escort. Mr. (now Sir Charles) Bright was appointed engineer-in-chief, and Mr. Whitehouse chief electrician.

The core of the first, as well as all subsequent cables, was made at the gutta-percha works in the City Road, and then removed in lengths of one mile each to the works at East Greenwich and Birkenhead, where it was covered with hemp and iron wire. The conductor of this cable consisted of seven copper wires, weighing 107 lbs. per nautical mile. The insulator was composed of three distinct coatings of gutta-percha, weighing 261 lbs. per nautical mile; this was again covered with hemp, saturated with a tarry mixture, hove taut round it, in order to increase the size and make a soft padding for the outer sheathing of iron wires to rest upon; the latter coating consisted of 18 strands, of seven wires each, No. $22\frac{1}{2}$ gauge. Therefore each mile of cable contained more than seven miles of copper wire, and 126 miles of iron wire, not including that taken up by the twist. Finally, this admirably constructed little cable was passed through a trough containing a hot compound of wax, tallow, and tar, which gave it a thick preservative coating. It was thus completed at a cost of £96 to £98 per statute mile; the weight of that length being one ton. Its specific

gravity, 2·95; weight of a nautical mile in water, 15 cwt.; diameter, 0·637 of an inch; and breaking strain, 73 cwt. The time required for signal to pass through 2,000 nautical miles of this cable was $2\frac{3}{4}$ seconds. The shore end was protected by 12 large iron wires, which made it $4\frac{3}{4}$ inches in circumference, and it weighed 8 tons per statute mile.

Shortly after the failure of 1857 (when 334 miles were laid from the Niagara), a very important discovery was made with regard to the conductivity of copper-wire, namely, that one specimen of the same size and appearance as another, would conduct the electric current 40 per cent. better. The cause of this difference depends partly on the purity of the metal, but the entire cause I believe to be yet unknown. The knowledge of this fact is taken advantage of practically, by testing every coil of wire which is taken to the works, against a piece which has been selected as a standard; and failing that point of perfection, it is rejected.

Mr. Willoughby Smith has kindly explained to me that the test is made by what is called Wheatstone's bridge, which is in this manner: the standard piece of determined length and weight (100 inches) is stretched across the table, the ends being made to connect with opposite poles of a small battery. The piece of wire under test, being precisely the same weight per length (but not cut), is brought to the same connections: a delicate galvanometer, of a few grains only in weight, is placed between them. If the wires possess equal conductivity, the galvanometer will not be affected by the divided current; but should one wire be superior, the current on that side will disturb the small magnet, in which case the piece under test must be shortened or lengthened till equilibrium is established, when, being carefully measured, for every inch it is more or less than a hundred, it will be one per cent. superior or inferior to the standard.

There is every reason to be satisfied with the conductivity and insulation of the present Atlantic cables, as a distinct signal can be transmitted in about ·2 of a second. The highest estimate I have seen, which includes the time required for a practiced eye to perceive it, is ·315 of a second, which is equal to 5,960 miles per second, or the circumference of globe—21,600 nautical miles—in $3\frac{1}{2}$ seconds. Messages are now transmitted through either cable at the rate of 12 or 14 words a minute, and no doubt will be sent still faster when required. The mirror galvanometer is now used for receiving the signals. This instrument consists of a magnet half an inch in length (carrying a mirror one-third of an inch in diameter, cemented to one end), suspended by a fibre of unspun silk, in the centre of a coil of insulated copper-wire; outside of which is placed a powerful bent magnet, which governs its position when uninfluenced by the electric current passing through the coil. Sir W. Thomson has had magnets and mirrors combined, weighing less than a grain, but those in general use weigh about $3\frac{1}{2}$ grains. The spot of light is thrown both right and left on Steinheil's system, the same as that generally used at railway stations: to the right signifies a dot, and to the left a dash, of Morse's code.

Before describing the cables of 1865 and 1866, it may be well to say a few words upon the financial difficulties encountered and overcome by the projectors. The £350,000 first raised, and the subsequent issue of £70,000, were entirely absorbed by the efforts made in 1857 and 1858; hope was almost extinguished, except in a few indomitable minds, till the spring of 1865, when £600,000 was raised upon 8 per cent. preference shares. After the temporary failure of that year, many of the gentlemen best acquainted with the circumstances were more sanguine than ever; but the difficulty of raising more money had also increased. An attempt was made to issue 12 per cent. first preference shares, but that was pronounced to be illegal. Therefore it was devised to form a new and distinct company; for though the directors had not power to issue new preference shares, they could employ the new company to complete the work upon still more advantageous conditions. Consequently, a few gentlemen met and formed the Anglo-American Telegraph Company; ten of them subscribing £10,000 each, and ten others smaller sums; the Telegraph Construction and Maintenance Company, £100,000, making a total of £230,500 at once before it was opened to the public. The whole £600,000 was subscribed in a few days, and the machinery in the City Road and at East Greenwich was again in motion night and day, preparing for a final effort. Had that failed, this generation would not have had telegraphic communication with America.

On reverting from the financiers to the civil engineers and their cable, we must also go back to the spring of 1865; we shall then find that since the failure (electrically) of the first Atlantic cable in 1858, Mr. (now Sir Samuel) Canning, Mr. Henry Clifford, with Mr. John Temple and others, under the direction of Sir Richard Glass, had been unceasingly testing the merits of every imaginary kind of cable, in order to choose that best adapted to the various requirements of a deep-sea cable. Those gentlemen commenced the investigation with minds well prepared, both by study and matured experience in ocean telegraphy. Similar ingenuity was applied to the choice and perfecting of the admirable machinery, which I will presently attempt to describe.

The cable of 1865 differed from that laid in 1858 in many important particulars. For example, the conductor was formed of the same number of wires, *i.e.* 7, but much larger; they weighed 300 lbs. per nautical mile; that of 1858 was 107 lbs. The diameter of the strand was ·14 inch. The insulation was also far superior: the conducting strand was first covered with "Chatterton's compound" applied hot, then four distinct coatings of the best gutta-percha, with a thin covering of "Chatterton's compound" between each. The entire insulator weighed 400 lbs. per nautical mile, which, with the conductor, made the entire core 700 lbs. per nautical mile: it was then ·464 of an inch in diameter, = 1·392 in circumference. The core was covered with jute as a protection, before receiving the external protecting strands, which for that cable were made of manilla, saturated with a dark preservative compound, each strand enclosing a steel wire of No. 13 gauge = ·095 inch. When thus complete its diameter was 1·07 inch, weight $35\frac{3}{4}$ cwt. per nautical mile, specific gravity 1·77,

weight in water 14 cwt. per nautical mile, and the breaking strain $7\frac{3}{4}$ tons. The length actually shipped was 2,300 nautical miles, weighing 4,598 tons, including the two shore ends.

The cable made during the spring of 1866, differed from the above only in the outer strands, which were of the best white manilla, enclosing homogeneous wire, No. 13 gauge, galvanized, the latter being considered less liable to spring or break. The breaking strain was increased to $8\frac{1}{10}$ tons, and the weight reduced to 31 cwt. per nautical mile, but in water it weighed $\frac{3}{4}$ cwt. more,$=14\frac{3}{4}$ cwt. per nautical mile.

The Valentia shore end of 1865 had the same core as the ocean part, protected by 12 strands of No. 4 wire = 36 wires, it was 27 miles in length, and weighed 18 tons per nautical mile = 486 tons, which was sent in a separate vessel.

The Valentia shore end for 1866 was protected by 12 solid wires or rods, ·44 inch in diameter, doubly served with yarn and coated thickly with a compound of pitch, tar, and silica, under Bright and Clark's process, till the diameter of the whole cable was 2·5 inches, its weight was about 23 tons per nautical mile. The Newfoundland shore end was only 3 miles in length.

The cable of 1857 was carried by two ships; the portion for the "Agamemnon" was 1,250 statute miles, it was coiled in the main-hold, which afforded a space measuring 48 feet by $44\frac{1}{2}$ feet, and 14 feet deep for the coil, in the centre of this space was placed a solid wooden cone. In the following year the size of the coil was slightly diminished and made more circular. The main-hold then received $1,129\frac{1}{2}$ statute miles in $216\frac{1}{2}$ flakes, the upper having 366 turns (or sheaves) each, equal to $6\frac{1}{2}$ statute miles. There were also 233 statute miles on the upper deck, and 95 miles in the fore cockpit; total $1,457\frac{1}{2}$ statute miles, equal to $1,265\frac{1}{2}$ nautical miles. This cable was kept in a dry state. The total weight carried by the "Agamemnon," including ship's furniture, was 2,804 tons.

The cables of 1865-6 were stowed on board the "Great Eastern," in three watertight iron tanks, having vertical sides; the foremost tank was 51 feet 6 inches in diameter, the main 58 feet 6 inches, the after one 58 feet, each 20 feet 6 inches in depth.

In the foremost tank was coiled 633, in the main 817, and in the after tank 803 nautical miles. These tanks being fitted with water cocks for supply and escape, the cable was kept continually in water, as in large pickling jars, the quantity of water was estimated at 1,200 tons. The ends of the cable were carried from each tank to the testing-room and joined, so that the tests could be carried on through the whole length unceasingly, whether paying out or at rest. Flexibility and adaptation to coil easily is a great desideratum when selecting a cable, so that it be not too springy to lie flat and even, that it has not too much tar or other substance which might cause it to stick, or long fibres of hemp to entangle its adjacent part and cause it to be displaced before required.

The machinery required to govern the egress of the cable underwent several changes and modifications before arriving at the present

state of perfection. The heavily covered cables for shallow seas, the "shore ends," &c., require a different and much stronger controlling power, than does the lighter but greater length which crosses the ocean. It is the latter only, as applied to the Atlantic cable which I will attempt to describe.

In 1858, two grooved drums or wheels were used, 6 feet 1 inch in extreme diameter, 9¾ inches wide, having four grooves, and placed 7 feet 4 inches apart, were they coupled together and attached to admirably powerful brakes (by Mr. Appold) as well as to a small engine, for the purpose of heaving back if necessary. The cable was passed round those wheels (not in a figure of 8 as in 1857) four times, there being 30 feet of cable to each turn. These sheaves or grooves increased ⅜ of an inch in circumference each, in order to take up the slack caused by stretching and to prevent slipping; but in practice it was found to force that amount of stretch from the cable and break it. After the experiments made in the Bay of Biscay in June, 1858, all the grooves were made precisely similar in diameter. This arrangement of machinery is very powerful for retarding the egress of the cable, and well adapted to a uniform strain, but it has too much inertia to meet the exigencies of strains and speed ever varying; manual effort was frequently employed when the strain was slight and irregular; one remarkable instance occurred when passing the shallow water off the coast of Ireland.

The machinery employed for the same purpose in 1865-6 was very different, the principal modifications having been proposed and carried out by Mr. Henry Clifford. As the machinery of 1865 was merely strengthened and adapted to heaving in over the stern, during the spring of 1866, it will be sufficient to describe it as it was when laying the cable in the summer of the latter year.

After the cable left the fair leaders from the tank, it passed over six grooved wheels about 3 feet in diameter, each connected with a brake, wheel by the same shaft, the cable merely passed over them, there was no turn. Above each of those wheels was placed one of half the diameter, having its periphery flat and covered with an india-rubber belt; these are called "jockeys" from the fact of their riding on the cable.

Those wheels are also connected with brakes, weighted at pleasure. As each pair of wheels revolve reluctantly, according to the weights on their respective brakes, the restraining power may be made great, but the moment it exceeds the desired limit it slips through the whole of them; and any number can be put permanently out of use: usually, only 2 or 3 were in action at a time. Also the whole of the upper wheels are connected by chains to one common shaft and that to two small wheels—like steering wheels, a small motion of either will lift the whole of the "jockeys" and allow the cable to run freely—but they have no power of restraining it. The man at the foremost wheel is on the look-out that broken wires should run past freely, while the man at the after-wheel watches the dynamometer, so as to guard against any sudden strain from without.

After passing those six pairs of wheels, it had four turns round a

weight in water 14 cwt. per nautical mile, and the breaking strain $7\frac{3}{4}$ tons. The length actually shipped was 2,300 nautical miles, weighing 4,598 tons, including the two shore ends.

The cable made during the spring of 1866, differed from the above only in the outer strands, which were of the best white manilla, enclosing homogeneous wire, No. 13 gauge, galvanized, the latter being considered less liable to spring or break. The breaking strain was increased to $8\frac{1}{10}$ tons, and the weight reduced to 31 cwt. per nautical mile, but in water it weighed $\frac{3}{4}$ cwt. more, $=14\frac{3}{4}$ cwt. per nautical mile.

The Valentia shore end of 1865 had the same core as the ocean part, protected by 12 strands of No. 4 wire = 36 wires, it was 27 miles in length, and weighed 18 tons per nautical mile = 486 tons, which was sent in a separate vessel.

The Valentia shore end for 1866 was protected by 12 solid wires or rods, ·44 inch in diameter, doubly served with yarn and coated thickly with a compound of pitch, tar, and silica, under Bright and Clark's process, till the diameter of the whole cable was 2·5 inches, its weight was about 23 tons per nautical mile. The Newfoundland shore end was only 3 miles in length.

The cable of 1857 was carried by two ships; the portion for the "Agamemnon" was 1,250 statute miles, it was coiled in the main-hold, which afforded a space measuring 48 feet by $44\frac{1}{2}$ feet, and 14 feet deep for the coil, in the centre of this space was placed a solid wooden cone. In the following year the size of the coil was slightly diminished and made more circular. The main-hold then received $1,129\frac{1}{2}$ statute miles in $216\frac{1}{2}$ flakes, the upper having 366 turns (or sheaves) each, equal to $6\frac{1}{2}$ statute miles. There were also 233 statute miles on the upper deck, and 95 miles in the fore cockpit; total $1,457\frac{1}{2}$ statute miles, equal to $1,265\frac{1}{2}$ nautical miles. This cable was kept in a dry state. The total weight carried by the "Agamemnon," including ship's furniture, was 2,804 tons.

The cables of 1865-6 were stowed on board the "Great Eastern," in three watertight iron tanks, having vertical sides; the foremost tank was 51 feet 6 inches in diameter, the main 58 feet 6 inches, the after one 58 feet, each 20 feet 6 inches in depth.

In the foremost tank was coiled 633, in the main 817, and in the after tank 803 nautical miles. These tanks being fitted with water cocks for supply and escape, the cable was kept continually in water, as in large pickling jars, the quantity of water was estimated at 1,200 tons. The ends of the cable were carried from each tank to the testing-room and joined, so that the tests could be carried on through the whole length unceasingly, whether paying out or at rest. Flexibility and adaptation to coil easily is a great desideratum when selecting a cable, so that it be not too springy to lie flat and even, that it has not too much tar or other substance which might cause it to stick, or long fibres of hemp to entangle its adjacent part and cause it to be displaced before required.

The machinery required to govern the egress of the cable underwent several changes and modifications before arriving at the present

state of perfection. The heavily covered cables for shallow seas, the "shore ends," &c., require a different and much stronger controlling power, than does the lighter but greater length which crosses the ocean. It is the latter only, as applied to the Atlantic cable which I will attempt to describe.

In 1858, two grooved drums or wheels were used, 6 feet 1 inch in extreme diameter, $9\frac{3}{4}$ inches wide, having four grooves, and placed 7 feet 4 inches apart, were they coupled together and attached to admirably powerful brakes (by Mr. Appold) as well as to a small engine, for the purpose of heaving back if necessary. The cable was passed round those wheels (not in a figure of 8 as in 1857) four times, there being 30 feet of cable to each turn. These sheaves or grooves increased $\frac{3}{8}$ of an inch in circumference each, in order to take up the slack caused by stretching and to prevent slipping; but in practice it was found to force that amount of stretch from the cable and break it. After the experiments made in the Bay of Biscay in June, 1858, all the grooves were made precisely similar in diameter. This arrangement of machinery is very powerful for retarding the egress of the cable, and well adapted to a uniform strain, but it has too much inertia to meet the exigencies of strains and speed ever varying; manual effort was frequently employed when the strain was slight and irregular; one remarkable instance occurred when passing the shallow water off the coast of Ireland.

The machinery employed for the same purpose in 1865-6 was very different, the principal modifications having been proposed and carried out by Mr. Henry Clifford. As the machinery of 1865 was merely strengthened and adapted to heaving in over the stern, during the spring of 1866, it will be sufficient to describe it as it was when laying the cable in the summer of the latter year.

After the cable left the fair leaders from the tank, it passed over six grooved wheels about 3 feet in diameter, each connected with a brake, wheel by the same shaft, the cable merely passed over them, there was no turn. Above each of those wheels was placed one of half the diameter, having its periphery flat and covered with an india-rubber belt; these are called "jockeys" from the fact of their riding on the cable.

Those wheels are also connected with brakes, weighted at pleasure. As each pair of wheels revolve reluctantly, according to the weights on their respective brakes, the restraining power may be made great, but the moment it exceeds the desired limit it slips through the whole of them; and any number can be put permanently out of use: usually, only 2 or 3 were in action at a time. Also the whole of the upper wheels are connected by chains to one common shaft and that to two small wheels—like steering wheels, a small motion of either will lift the whole of the "jockeys" and allow the cable to run freely—but they have no power of restraining it. The man at the foremost wheel is on the look-out that broken wires should run past freely, while the man at the after-wheel watches the dynamometer, so as to guard against any sudden strain from without.

After passing those six pairs of wheels, it had four turns round a

smooth-faced light drum, about 6 feet in diameter, which also had brakes attached to it. This drum could be coupled quickly with strong gearing and a powerful engine, for heaving in over the stern.

Before the cable passed over the stern-sheave, it was brought by fair leads to a perfect level, and under the roller of the dynamometer, which rode as it were upon it, and, being weighted in proportion to the scale used—the pointer would indicate the exact strain. A similar instrument was used for the grapnel-rope.

A description of the dynamometer may be interesting to those who have not seen its admirable working. The roller or wheel is secured to a slide box, which is confined between smooth-faced guides, free to rise and fall vertically, while the wheel revolves on the cable, or grapnel-rope, thus bearing it down, but not retarding its progress. The slide is connected by a rod to a piston, which works freely in a cylinder filled with water, in order to steady its motions. The slide is weighted to the desired extent, thus with a strain on the rope of 60 cwt. the points of suspension being 30 feet asunder and 1,484 lbs. on the slide—including its own weight with roller, &c.—the indicator would be depressed nearly 20 inches, the cable having been drawn down 6° 18′ below the horizontal line. The proportion is $\frac{\text{Rad.} \times \text{depression}}{\text{half distance}} = \text{Tang}\angle$, and $\frac{\frac{\text{Weight}}{2} \times \text{Rad.}}{\text{Sine}\angle} = \text{Strain.}$ Thus, $\frac{\text{Rad.} \times 19{\cdot}87}{180} = \text{Tang } 6^\circ\ 18'$, and $\frac{742\text{lbs.} \times \text{Rad.}}{\text{Sine } 6^\circ\ 18'} = 60\frac{1}{2}$ cwt. An allowance must be made for a diminution in the distance between the points of suspension as the angle of depression increases according to the diameter of the rollers over which the rope passes. When the angle increases beyond the intended range, the weighted slide rests on its seat, simply indicating that the strain is less than the lowest figure on that scale. If necessary, the weights can be reduced to some other predetermined amount, and a corresponding scale applied. During every stage in the process of grappling, as well as when laying a cable, this auxiliary is indispensable.

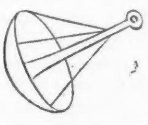
The "picking-up machinery" used during the summer of 1866, consisted of two large drums, of similar diameter, with smooth faces, sufficiently broad to take four parts of the grapnel-rope. These drums were placed near the fore part of the ship, about twice their own diameter apart, controlled by strong brakes, and coupled by suitable gearing to a pair of Penn's trunk-engines when desirable. The rope did not pass round those drums in "a figure of eight," but simply round them both. A set of guide rollers between the two drums, acted upon the lower turns as they passed, shunting them into the desired positions, whether heaving in or paying out. Abaft the drums was a jockey pulley for drawing the rope off as it was hove in. On the fore side was the dynamometer, under which the rope passed before going to the fair lead over the bow.

The grapnel-rope was composed of 245 yarns of the best Manilla,

and 49 steel wires. Five yarns were laid round each of seven wires, and those twisted together formed a strand, arranged similarly to the seven wires of the conductor in the cable. Seven of those strands were again laid up in like manner, one forming the heart. The wires were of homogeneous iron or steel, of No. 13 gauge, ·095 inches in diameter, and galvanized. When complete the rope measured 2·125 in diameter, and 6·676 inches in circumference. It weighed 125·158 cwt. per nautical mile (in air), and 56·504 cwt. in water; the specific gravity being 1·823. The breaking strain was 30 tons. The "Great Eastern" carried 7½ nautical miles of this rope in one length; the "Medway" a similar piece, and the "Albany" one length of 5 miles. It was found to answer the intended purpose admirably; the only point in which improvement could be desired was that it should not unlay itself when under a heavy strain. Swivels were interposed at intervals (where most needed) after it had been used, but they are attended with inconvenience.

The principal part of the buoy-rope provided was of similar character to the above; homogeneous wire, galvanized, and manilla. There were 16 wires, No. 13 gauge. The weight per nautical mile was 43·298 cwt., and in water 18·008 cwt. Its strength was equal to 13 tons. Part of the rope for securing the "mark-buoys" contained only 9 wires, and the lower portion of each was coir, 6 inches in circumference, equal to a strain of about 3 tons. The coir rope is less liable to kink when dragged about the bottom, than the stiff heavy rope, but the lateral force of the current upon the broad rough surface was found in some instances sufficient to overcome the holding power of the anchor, causing the buoy to drift till the current became slack, when it would again take hold. There was also a chain 20 or 30 fathoms in length, attached to the anchor for each buoy.

The mushroom anchors which were used last summer weighed 2, 3, and 5 cwt. This is the only description of anchor which can be used near a cable, for fear of hooking it; and it appears possible that they could be rendered more incapable of dragging a cable, by placing four or five rods of iron from the shank to the outer edge, thus:—



which would not prevent their filling with sand and mud, or sinking in either.

The buoys were of three sizes, but similar in shape, and made of thin iron, by Brown and Lennox. The views given of them in the *Illustrated London News*, by Mr. Dudley, is better than any description which I can give. Those to be used as "mark-buoys" were 7½ feet in height, 6 feet at the base, and 6½ feet in diameter at the widest part. Each weighed 25 cwt., and was capable of supporting, besides its own weight, 1 ton 13 cwt.

The "bight-buoys" were $9\frac{1}{2}$ feet high, 7 feet in diameter at the bottom, and 7 feet 8 inches at the broadest part; the weight was 30 cwt.; immersion when light, 1 foot 3 inches; when loaded with $3\frac{1}{2}$ tons, 3 feet.

The buoys for grapnel-rope were 12 feet in height, 9 feet 6 inches in diameter at the bottom, and $10\frac{1}{2}$ feet at the widest part. Weight, $2\frac{1}{4}$ tons. Immersion when light, 1 foot $0\frac{3}{4}$ inch. Displacement when immersed to 5 feet 10 inches = 10 tons.

Each of these buoys was fitted with a stout eye at the centre of the base to receive the mooring chain, the end of which was secured by a tongue-slip at the side. "The bridle," whereby the mooring and load was transferred to the ship, was secured on the opposite side. They had also chain slings round them ready for being hoisted out or in. Each buoy carried a staff 15 or 30 feet high, surmounted by a ball and flag, by which they could be seen 6 or 8 miles; they were most conspicuous when painted dark red. These buoys were found very efficient in every respect, the result of mature calculation on the part of the civil engineers.

The choice of a ship to carry an ocean cable is a very important point. In the first place she must have capacity to stow the required length and bear the weight, especially as it is now deemed necessary to keep the whole submerged in water. The weight and space occupied by machinery is also considerable. The pitching motion does not appear to be so injurious while the cable is running out, as was originally conjectured; but it is a motion highly detrimentral during repairs, or when grappling. The rolling motion of the "Agamemnon" on June 1st, 1858, disturbed the upper portion of the main coil; and a hundred miles were hauled up by hand, in order to recoil it. That might have been prevented by stowing sails or some light substance on the top: it is usual now to place a surface covering of plank, and to shore it down from the deck. The slight motion experienced in the "Great Eastern" during moderate weather, is one of her principal recommendations as a cable ship, as also the advantages derived from the employment of one large ship in preference to two smaller ones; but I believe the "Great Eastern" was enormously expensive.

When two ships are employed and they commence in the centre, as they did in 1858, they insure the great advantage of being able to choose the time, and most favourable weather for commencing, with which, in a few days, they would pass over the most dangerous part of the ocean. In 1858 the two ships paid out with great regularity; on July 30th, 10.40 a.m., "Niagara" had paid out 200 miles, and at 11 a.m., "Agamemnon" 200. August 2nd, when "Niagara" reported 750, "Agamemnon" $748\frac{1}{2}$ miles. "Agamemnon" was ten minutes late with her 800, and the "Niagara" landed her end one or two hours before "Agamemnon" did hers.

That one ship should commence to pay out her half from the shore, to be joined by that in the second ship—as intended in 1857—is quite practicable under ordinary circumstances, but a heavy gale at that particular time would be hazardous to some degree, probably necessitating the slipping and buoying of the cable. If the cable is to be laid

between two points already in telegraphic communication, it is decidedly preferable to commence from the shore, as valuable information could be exchanged between the ship and the shore she approached.

As the requirements of a cable-ship are peculiar, at no distant date it may be deemed desirable to build one or two specially for that purpose, having both strength and capacity for the enormous tanks, which would then be interwoven with the structure of the ship. As capacity and stability would be the chief requirements, that bugbear speed, which destroys the seaworthiness of so many ships, need not enter largely into the calculation. She should also be well rigged, in order to economise fuel when not laying the cable, and, with good sails at the extremities, to assist all manœuvres. I wish to express in the strongest terms possible my dissent from the present system—almost general—of sending ships to sea entirely dependent upon their steam power and their rudders; should anything happen to either, they become logs, at the mercy of the winds and waves. Masts tend to counteract the effect of the dead weight below, and prevent that quick, disagreeable rolling motion which is experienced in vessels that are undermasted, and no seaman would willingly be without sail during a gale; for my own part I would not use steam during a gale in the open sea, except under some emergency.

Among the various considerations in fitting out a cable ship, none are more important with a view to her correct steering and careful navigation, than her magnetic character and compass requirements, as in all probability she will be constructed of iron, and having to carry a cargo partly composed of the same metal, which is ever varying in quantity during the most important period; while at the same time altering her geographical position and consequent magnetic condition, the problem it will be seen, becomes somewhat complicated. It is only by careful study and a sound understanding of the principles and laws which govern such effects that their evil consequences can be obviated, commencing with a thorough knowledge of the magnetic properties of the ship herself, from the time of her launch till about to commence her voyage.

As those fundamental principles, especially with reference to the "Great Eastern," had been thoroughly investigated by Staff Commander Frederick Evans, R.N., F.R.S., they were applied in the most practical manner in the summer of 1865, to the correction of what became the principal navigating compass in the "Great Eastern." It was one of the "standard compasses" as supplied to Her Majesty's ships, and placed in a position which was both the best and most convenient that could be found; but that was only 14 feet abaft the second funnel, and 13 feet 8 inches before an iron-mast. After a few azimuths had been taken with this compass, and the deviation found to be 32° 30′ E., with her head E.N.E., and 28° 40′ W., with her head W. by N., Captain Evans, from his office, sent a single bar magnet, with directions for it to be placed $17\frac{1}{4}$ inches below the centre of the compass card, with its north pole pointing 5° to starboard; with the intimation that the maximum deviation would be about 5° at the quadrantal points. The magnet

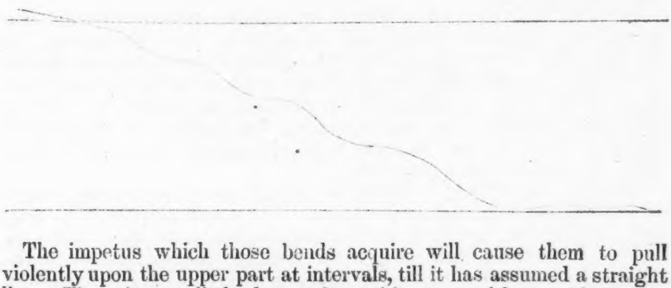
was so placed and the deviation immediately reduced to a few minutes only at three of the cardinal points, nothing at north, and 4° to 6° was the maximum in the quadrants. Mr. Mayes (the Admiralty Superintendent of Compasses), visited the ship to make any necessary alterations, but none were required.

The magnetic influence of the ship upon the compass changed during the voyage from Sheerness towards Newfoundland, proportionally as the dip of the needle increased, and the earth's horizontal force decreased, causing changes in the deviation, as would be known to follow and might be anticipated, but by constant azimuths the changes were allowed for. That this subject is often much neglected, I can adduce no better example than that of the largest ship in the world having an iron bar placed within 2 feet of what was her principal compass, prior to this voyage. The majority of our merchant ships have compasses of an enormous size, very imposing in appearance, from the quantity of brass, boxes of chain, magnets, &c., but generally very sluggish, and inconveniently fitted for taking azimuths or bearings correctly. The choice of position with regard to the magnetic properties of the ship appear to be but seldom studied.

The process of paying out the cable has been necessarily touched upon in describing the machinery by which its egress is controlled; those points need not be repeated. The speed of the ship must be regulated according to the weight of the cable and depth of water. The shore-ends are usually very heavy, as much as 18 tons per nautical mile, but the depth of water being comparatively small, they can be laid at the rate of three or four miles an hour, with very little slack. An ocean cable which is to be laid in deep water, 2,000 fathoms, for example, and it being of similar specific gravity (1·77) to the Atlantic cable, would require considerable time to reach the bottom, and an amount of slack must be allowed, in order that it may do so without strain. A speed of 5 knots for the ship, while the cable runs out at the rate of $5\frac{1}{2}$ knots, or about 10 to 14 per cent. on the distance, has been found safe and sufficiently fast.

The manner in which the cable passes from the stern of the ship to the bottom of the ocean, and how it adjusts itself there, with regard to the inequalities of the surface, and its own superabundant length, has given rise to much discussion. It appears to me impossible that it can run down uniformly in an inclined plane, and then assume a straight line at the bottom, though these suppositions have been maintained by such competent authorities, that I most reluctantly dissent from them. In the first place the cable enters the water at an angle of 10° or 12° from the horizontal, which, if continued in an uniform line, would touch the bottom at $9\frac{1}{2}$ to $11\frac{1}{3}$ miles, and a catenary curve would extend to about 15 miles; but I cannot imagine that it assumes either, for if it sank laterally, preserving the same angle there would be no means of disposing of the slack cable; but as there are from 200 to 300 fathoms surplus upon every mile run, it must dispose of itself in bends on reaching the bottom; and as considerable torsion exists at the surface, there is no doubt a little torsion towards the bottom, and those folds would be thrown uniformly towards one side. What it

does with itself at the bottom is of minor consequence, unless when requiring to be picked up, but the manner in which it reaches that state of rest may affect its safety. When a cable is large and rough upon the surface, with low specific gravity, the friction will be great, and its motion will be softened and rendered easy of control, but the irregular and uncertain motion of a comparatively heavy cable (in point of gravity), such as that used in 1858, greatly compromises its safety. On June 29th, 1858, at 9.20 p.m., when near the centre of the Atlantic, the speed of the "Agamemnon" was reduced from 5 to 3 knots, the strain having previously been 19 to $21\frac{1}{2}$ cwt., and the vertical angle $12\frac{1}{2}°$ to $15°$, depth about 2,000 fathoms, and cable paid out $146\frac{1}{2}$, to a distance of 118 miles, which is 24 per cent. of slack. For a space of ten minutes after the speed was reduced the cable continued to run out slowly, but very irregularly, giving a strain upon the dynamometer of $20\frac{1}{2}$ cwt. from zero in an instant. In one of those sudden pulls which I witnessed, it broke on the stern-wheel, it was too sudden to reach the dynamometer, and certainly was not caused by the pitching or any motion of the ship. The solution I then arrived at, and still believe, is that the cable does not sink to the bottom at any uniform angle, but entering the water at 10° or 12° only from the horizontal, it sinks very slowly, till it assumes irregularities of angle or convolutions, which by bringing their length to coincide with the direction of their gravitation, they sink rapidly, forcing the upper parts to follow their section, and thus absorbing the minor folds above, increasing in size and velocity till they reach the bottom, thus:—



The impetus which those bends acquire will cause them to pull violently upon the upper part at intervals, till it has assumed a straight line. There is very little danger from this cause with a cable similar to that used in 1865 and 1866, as it has only a little more than half the specific gravity of the former, and is comparatively large and rough, the strength also being equal to a strain of $7\frac{3}{4}$ or 8 tons; the former was only 73 cwt.

The want of uniformity in the proportion of slack to the distance run from day to day, was a source of some perplexity and disappointment, when the weight upon the brake levers, the motion of the machinery, and the speed of the ship were similar to the previous day, and the observations each day supposed to be good. But immediately the existence of a varying current is admitted, the apparent

disagreements are reconciled. For the cable is not fast to the bottom and paid out like a measuring tape, or "ground-log," but is rather comparable to the line paid out after the ordinary log-ship, which can take no cognisance of current. If the current be lateral, it will not affect the question, but should it run with the ship at the rate of half a mile an hour, it will carry her an extra 10 miles while she logs 100, and the indicator shows 116 miles of cable, which, instead of being $\frac{16}{100}$ of slack, will only be $\frac{5 \cdot 45}{100}$, or $5\frac{1}{2}$ per cent. nearly. Whereas if a current of similar strength were adverse, it would produce the most unsatisfactory result at the end of 20 hours: 90 miles of distance to 116 paid out, or 28·9 per cent. of slack. Nor do I see any means of avoiding this apparent discrepancy, or of discovering it, except at comparatively long intervals of time; and if known, the only remedy would be by increasing the strain on the cable. All the cases of disagreement between the reckoning and the cable paid out which I have met, were in my opinion attributable to that cause alone. This is an important consideration, when about to pick up a cable for repair, in order to choose a place where there is much slack. It is very important that cable ships should be well furnished with patent logs, and every means of ascertaining their run correctly.

The cable itself will assist the navigator very materially by pointing out to him the amount of leeway she is making, an arc and pointer being placed over the last lead of the cable for the purpose. The angle so shewn was strictly applied as leeway, in the "Agamemnon," in 1858, and her track was very close to the great circle. It is surprising to see how little wind on the beam, or even on the quarter, will cause leeway.

The task of picking up a cable in deep water is more difficult than laying one, therefore details may probably be more interesting. In the first place the position of the lost cable must be accurately known, and unless a considerable decrease in the depth of water at some other place should cause a preference, the attempt should first be made at about four miles from the end, supposing the depth to be about 2,000 fathoms, not only for the sake of economizing cable, but it may there be raised by one or two lifts.

The first "mark-buoy" should be placed two or three miles within the end of the cable and close over it. For which purpose it is desirable that the ship should be as close as possible to the spot at 6 a.m., or when the sun is high enough for reliable observations,—if in the Atlantic, a westerly wind is preferable, as it gives a clear horizon,—by renewing those observations every hour or two till noon, and correcting her position by small runs as soon as the requirement is known, she must be very close to the spot, and also a good estimate may be formed of the drift, by the time of obtaining the meridian altitude; after which the sooner the buoy is down the better. As one or two hours may be occupied in lowering it, allowance must be made for drift. The afternoon sights will again check and correct its position, both for latitude and longitude. In case the desired accuracy cannot be insured at the first attempt, I would advise an auxiliary mark-buoy to be placed if only within three or four miles of the desired position,

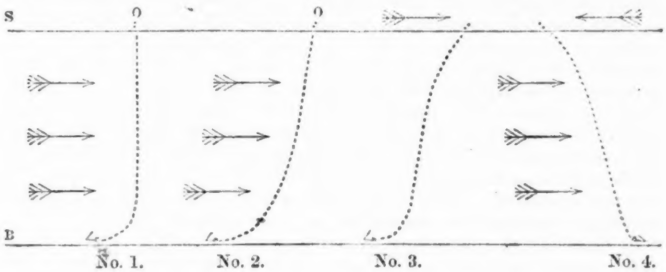
and others built, as it were from that. As the buoys in 1865 and 1866 were moored with $2\frac{1}{4}$ miles of rope in two miles' depth of water they were liable to change position half a mile on either side the spot directly over the mushroom. One reason why good observers sometimes disagree considerably from each other, is that the error inherent to every sextant, through a very slight and unavoidable defect in the centreing, is very seldom allowed for. The excellent work by Mr. Sims upon the sextant, is the only publication I know, wherein it is mentioned.

While mentioning some of the considerations connected with grappling, the large rope of 1866 only will be spoken of. When lowering the grapnel it is necessary to weigh it and the rope (by the dynamometer) at various depths, in order to check previous calculations and to form a practical scale for its normal condition at every depth. Last year when the depth of the sea was about 2,000 fathoms, from 2,200 to 2,300 fathoms of grapnel-rope were used, the strain while dragging was then about $8\frac{1}{2}$ to 9 tons. An increase of one ton steadily, would indicate that the cable was on it. On August 17th, 1866, the bight of cable was hove 10 feet above the surface, the strain then as shewn by the dynamometer was $6\frac{1}{2}$ tons, which Professor Sir William Thomson attributes entirely to the weight of two catenary curves, 4·64 of a mile each, forming an angle at the apex of 85°. The base, or space, from whence the cable had been lifted was supposed to be a little over eight miles. But it was impossible to tell the exact angle, although in sight, or the amount of slack at that particular spot, or how much the cable might slip over the soft bottom. The ship was rolling more than usual at that time, and the direction of her head was at a broad angle with the span, therefore it was strained upon the claw of the grapnel in alternate directions. The greatest strain on that occasion, with grapnel-rope and cable combined, was $11\frac{1}{2}$ tons. When the grapnel was 800 fathoms from the surface with the cable on it, the dynamometer shewed 7·4 tons, which would indicate a strain of nearly 4 tons on the span of the cable, which was then 1,100 or 1,200 fathoms off the bottom. On another occasion when the bight was 1,000 fathoms from the surface, the strain was $10\frac{1}{4}$ tons, consequently there was a strain of six tons nearly on the cable, three tons to each part; the monster buoy rode by it for an entire week, and was only set adrift by the cable being broken (by grapnels) on either side of it. It was upon that occasion that the piece of cable $1\frac{8}{10}$ miles in length was hove on board.

It is very desirable that the grapnel should be lowered two or three miles from the cable, on the side from whence the current may be running at the time; the direction of a light wind is of less consequence. It is frequently very difficult to choose the proper side, or to ascertain the direction of the main current, till the grapnel is down, when, if anticipations are found to be wrong, there is but little hope of doing any good by means of steam or sail. The alternative of heaving it up, getting into position, and lowering the grapnel again, would nearly occupy the day. Though the operation was completed last summer by September 2nd, the days were never so long as we desired. The

time of lowering the grapnel in 2,000 fathoms occupied about $1\frac{1}{2}$ hour, and 2,200 fathoms, or 2,250 fathoms of rope and chain, were found sufficient at that depth. The rate of drift should be less than a mile an hour.

The following diagram is intended to show the manner in which the grapnel-rope is supposed to be affected by the current, when drifting with it, or being forced against it by wind, steam, or surface current. The arrows below the surface line indicate the direction of the current, and those above show the direction of the propelling power.



The line S represents the surface, B the bottom, two miles deep. No. 1 represents an uniform current the whole depth, and the vessel lying motionless on the surface. No. 2 also supposes the ship to be still, but the current stronger towards the surface. No. 3 supposes the water to be motionless, and the ship moving slowly. No. 4 represents the ship drawing the rope against the direction of the current, and the grapnel just slightly touching the ground.

During the summers of 1865 and 1866, when near the position of the principal operations—$52\frac{1}{2}°$ N. and 39° W.—the current ran more frequently to the N.E.—true—than any other direction, occasionally as much as three-quarters of a mile an hour; but during the few days the ships were about the second position—52° N. and 36° W.—the current continued to run to the southward.

Whenever it again becomes necessary to repeat the operation of recovering a cable, which had been laid with the usual amount of slack, from 2,000 fathoms, the plan adopted by Sir Samuel Canning in August, 1866, will admit of very little improvement. That is, by raising it half way at two or three points, before trying to lift one to the surface; unless it be almost as desirable to break it, and so make an end, as to get it in at once.

The durability of gutta-percha is believed to be so great, and the motion at the bottom of the ocean so slight, that there appears but little probability of the present generation of civil engineers being required to raise the Atlantic cables of 1865 or 1866 for repair. That these records of industry and science may last a hundred years, is not only my hope and desire, but my full expectation.

Captain LEOPOLD HEATH, R.N., C.B.: I wish to point out what appears to me a defect in the system adopted for laying this cable, a defect tending to form kinks. Every sailor knows that when you coil down a forebrace, if you lay hold of the end of it and walk away, you will see in the rope as many extra turns or twists as there are circles in the rope coiled down. That is precisely what is done in coiling down the Atlantic, or any other deep-sea cable.' The cable is coiled, or rather "flemished" down into the hold; coil upon coil are superimposed, always in the same direction, until you reach the top of the compartment, and then you shift to another. The consequence is, that when you make fast one end of that coil at Valencia, and steam away, you pay out from the ship as many extra twists or turns as there are circles in the hold of that ship. I am not sure that I caught the figures accurately, but I think it was said there were 216 sheaves, and in each sheave there were 316 circles, making in one single compartment 68,256 circles; so that, during the paying out of that one compartment, there were 68,000 extra twists (beyond the normal twist of the cable) paid out with it. Those 68,000 twists are 68,000 elements for the formation of kinks. I do not mean to say that they did form, we hope they will not form, the 68,000 kinks, or even one kink; but still I maintain that there are there, the elements to form kinks, and if other circumstances combine, such as a sudden check to paying out the cable, it is quite possible that kinks might thus be formed. There are but two ways that I know of, of avoiding this. One is, to coil on to a drum, and to pay out from a drum, as was done with the Varna cable. But that, I take it, is impracticable where more than one drum is necessary, because I do not know how you are to connect from one drum to another. It has been proposed by Captain Selwyn that it should be paid out from a single drum in the water; but that would be a great experiment, and I am not surprised it has not been tried. Still I believe the principle is correct, because it will avoid that one difficulty of forming kinks. The second plan would be to fake the cable. That I take it is also impracticable. It would take up too much room. The motion of paying out would be like that of an oscillating steam-engine, as compared with a rotary steam-engine. It would go backwards and forwards, and the bights at the end of the fake would be getting under one another. It would be utterly impracticable. The third plan is my own. It is that the sheaves should be coiled alternately right and left handed; I see no practical objection to that. The only thing would be that your men in the holds, if they did not know it, would be a little puzzled; but as they would be the very men who coiled the cable, I take it it would be a mere matter of drill, so that on each successive sheave being laid out, they should be prepared to reverse the motion. I have brought some pieces of tape, which will show the thing better than anything else. They are just the same as they came out of the shop, except that I have sewn two together. If any gentleman will represent Valentia, I will represent the "Great Eastern." This tape represents one sheave of the cable as stowed in the "Great Eastern." The upper end, of course, would be the end landed. If, Sir, you will be kind enough to represent Valentia, and hold that end of the tape, I will be the great ship, and will steam away and pay out. You will observe, although this is coiled quite flat, that for every turn that is taken out of the hold, there is a turn put into the cable. You see the turns plainly in that tape. You would not have seen it so plainly if I had brought you a piece of rope, and that is the reason I have brought you a piece of tape. Now this represents the way that I should recommend. (Producing another piece of tape differently folded.) This represents two sheaves, the first being coiled in one direction, and the other in the opposite direction. I land the upper end as before at Valentia, and I steam away, taking in as before the materials for one kink in every circle that goes out, until I come to the end of the first sheave. Unless I have made a mistake, and put my tape the wrong way, the next will take in an equal number of twists; but in the reverse way, therefore, on the whole, you will, as you see, have your cable laid out without kinks.

Captain SELWYN, R.N.: Before commencing my remarks upon the main subject, it would be extremely disrespectful if I did not say, what I feel most strongly, that however we may differ on the mode to be adopted in the true solution, or more ready solution of what has been one of the greatest problems of the age, yet there is one point upon which nobody can possibly have a diverse opinion, that is, the merit of

the undertaking, and the congratulation to be awarded to those who, in the face of danger, difficulty, and discouragement, of every kind, have persevered until they have brought about that magnificent union of the two hemispheres, which we are met here to-night to talk about. Whilst those who have had to do it, have passed through the difficulties, I hope they have learned a great deal from those very difficulties. I hope some of them will become better seamen than they were when they started, and that is only a brotherly wish; and I hope they will not be so subject to those evils which the sea brings upon those who embark upon it for the first time. But I think I do here record in this Institution the opinion of seamen when I say that no seamen could possibly have shown greater perseverance under difficulties, however much their previous knowledge might have enabled them to meet them better. Having thus awarded the meed of praise which everybody must feel is due, as far as I could do it, I must venture to dissent from some of the propositions that we have heard. I am sure that all scientific men will feel that the one of the greatest discoveries a scientific man can make, and the acknowledgment that does him the most honour, is, that he may have been mistaken. With regard to the question whether this generation would have seen the fulfilment of this great operation had it not been carried out by those who have been so fortunate as to do so, I think that is rather begging the whole question. Although we believe that they have shown the very greatest perseverance that was necessary for their object, yet we can scarcely believe that there are not others who might have had equal perseverance, and who might possibly have brought to the subject some more professional skill. Now the very first case in which that comes home to us is this, that whereas the conductor and the insulator of existing cables have been calculated carefully to give a certain speed per hour or per minute, which was supposed to be commercially valuable, and whereas they were calculated as low as possible, in order not to ask for more capital than the public were disposed to supply, the very first discovery made when the cable was laid, showed that we were yet learners in that respect, that a great deal more could be done with that conductivity and that insulation than anybody had supposed before to be possible, by what was really a simple, though very clever discovery of the charging of the cable: that is to say that the conditions under which the messages could be sent were found to be most favourable when they were utterly diverse from those which had previously been predicated. We must never cease to remember also in this question, and we are yet only on the threshold, that ropes are strong only comparatively, and that that rope which you see before you, a shore end, with an outer covering of wire, which looks as though it would resist every strain that could possibly be brought upon it, is in fact, of about one of the very weakest constructions, when called upon to support a great length of its own weight. The cause of the success of modern cables is not the reduced weight alone, but it is that the component parts have been reduced in specific gravity. By a large admixture of hemp, the specific gravity of the rope is less in proportion to the weight than would have been the case had we employed throughout, perhaps a stronger material, but one of greater specific gravity; that is to say, that whereas steel wire will bear about four miles of its own weight in air, fishing gut—which is infinitely weaker—fishing gut in tensile strength, is very much stronger relatively to weight, and will bear some twenty miles of itself hanging in air. Thus we see that the question of strength is solved by the deduction of the weight of the material employed from its tensile strength, the remainder being the whole of the tensile strength really available for your purposes. Now when we consider that a steel wire is also applied, as formerly, round the conductor and its insulator, the gutta-percha and copper wire, and that it has embedded itself in hemp, we see that by no possibility, if a great strain is to be borne by it, can the copper conductor be protected from all strain. The copper conductor will bear its share of the strain, and it will bear it first because of the difference of stretch due to spirals or due to straight lines. It is not that that will very materially affect the whole result, provided there be no very weak point in any part of the copper wire, but it will induce a certain amount of stretching, if there be that strain to be borne. It is not protected, physically speaking, from strain by the outer coating. A cable like that of Mr. Allan, which was once said to be like a man with his bones outside his flesh, or his brains outside his

skull, a notion with which I do not at all coincide, is really the strongest you can employ, surrounded as it is by a large proportion of material of lighter specific gravity. I think these ideas may have some value in regard to our future progress, and that we shall learn to distrust the deep-sea less, and to distrust the shallow water more than we do now. Reference has been made to danger by Captain Moriarty, who I must say has treated his subject from a point of view which I for one am most delighted to see adopted, that of a thorough seaman and scientific observer, and a man evidently thoroughly capable of dealing with it, so far as his knowledge went. He says that the magnetic influences of the ship are always to be feared. That is perfectly and absolutely true; not less in this instance than in that of our commerce generally. But as I have previously pointed out in this Institution, whenever we will take the simple plan of putting a compass so far above the ship as to be out of the field of magnetic influences, and consult that on occasions as a comparison, we shall cease to fear the bugbear of deviation altogether. Such a place is to be found in the ship at the head of the topgallant-mast, if not lower, and the compass may be there only for comparison, and it will always give true results. It does not matter one iota to me whether my watch keeps Greenwich time or not, so long as I have a chronometer with which to compare it. The magnets, and I speak here with a full knowledge that the question is a very wide one, and one which is probably the most fertile cause of loss and disaster, since the employment of iron ships, except, perhaps that question which was discussed here—the port and starboard helm; the magnets lead in many cases to error: not so much in such a case as the Atlantic telegraph cable, where you pass at once from one shore to another, on the same parallel, but very often where you change your latitude. In taking out cables to India, or to those parts of the globe where we shall be called upon to lay them, it would not be wise in a navigator to trust to his magnets for the correction of his compass without very close astronomical observations to confirm them. The question of the angle in laying the cable is one I have very often spoken of, and it is one in which I am happy to say Captain Moriarty has to a certain extent corroborated the views I have advocated. He has shown the angles at which a grapnel-rope will probably descend, when affected by currents; but the converse of the proposition is also true, that if a ship is passing over the ocean laying the cable, precisely the same operation takes place, as though the current of the water were against the cable; that is to say, curves are induced by the deposition of the cable on the water, probably with no strain other than that due to its own weight, and with a certain proportion of slack which can never be taken up in any other way than by curves. You will never get short curves. You will get long curves, due to the presence either of currents, or to the difference between the forward motion of the ship and the rate of sinking of the cable sideways, or by cutting descent. Therefore the curve will be an upward one in all but a very few cases. The mode of laying cables proposed by me has been alluded to as the best theoretical system, and with regard to that I am quite content to leave it to the decision of future generations. I believe it is the right plan, and I believe it will be adopted when people think it is not too great a novelty. But I must also speak a few moments upon the proposition of my friend Captain Heath. He has spoken of a means of stowing cables which he thinks will obviate the tendency which we all admit, without exception, exists, to form kinks, sometimes causing no very great damage, but occasionally showing themselves to an electrician's great regret, and forming one of his cabinet curiosities afterwards. I have seen them in many telegraph offices. Captain Heath proposes to stow the cable, in alternate layers, from right to left, and from left to right; but I must observe to you that when you deal with a cable of such a weight as telegraph cables are, and of such fragility as light ones are, and you have to deal with that cable going out, at a speed varying from three and four to six miles per hour, which is very rapid indeed; the chances of damage are very great. If a man sees a cable flying out at that rate, he is apt to think it is an excessively dangerous thing to interrupt its motion, and if he, at the end of a certain coil, suddenly changes its motion to an opposite direction, he will find that the forces brought into play are quite sufficient to ruin his cable. Recollect, you cannot govern the motion of a ship suddenly. The ship's speed cannot be slacked

to meet a sudden motion at all times. She may be going six knots, and suddenly there comes a motion of the cable in the opposite direction. Engineers know how dangerous a thing that is with heavy bodies, even with heavy bodies in slow motion. Now, bodies having high velocity may be light; but the force brought into play may be exactly the same as those of a heavy body in slow motion. There was a point also adverted to, viz., the connection from one drum to the other. I think electricians will thoroughly understand me when I say there is no difficulty whatever in that, that a coil on any possible drum, of any possible size, may be brought into electric connection with any other possible coil on a drum, by means of a friction apparatus. There is no difficulty at all about it; it is too familiar to dilate upon. Having said this much, I think we ought to ask some of the persons who have been joined in Captain Moriarty's exploit, to give us answers to questions which may arise during the discussion if they feel able to do so in Captain Moriarty's absence, as it is very desirable that no question should pass without the answer to it being given.

The CHAIRMAN: If no engineer connected with the great work is present, perhaps Captain Commerell, who commanded H.M.S. "Terrible," will address us.

Captain COMMERELL, R.N., V.C.: I am afraid I can give you very little information upon the subject. Commanding as I did a ship some distance off, I can, as you may be aware, say very little concerning it. But this I can say, that I think Captain Heath is very much mistaken about kinks. I am not aware that anybody observed any kinks over the stern. As I was in the "Terrible," astern of the "Great Eastern" on many occasions, the cable appeared to me to come out as straight as a line; I believe the cause of that was, that as the cable was coiled into the ship a kink was taken in, and as it went out, a kink was taken out. The old cable, when it was raised from the bottom, came up also as straight as a line; and I want to know, if it did not go out as straight as a line, what became of the kinks? Captain Moriarty only said he believed there were kinks, and that at the bottom. We can only speak of things as we see them, and I, certainly, when under the stern of the "Great Eastern" from day to day, and from hour to hour, never saw the slightest appearance of a kink go out. The cable appeared to go out like a silver line, and when the old cable was got up, there was certainly no appearance of kink at all. The only time I ever saw a kink in any one of the cables, was in the Saint Lawrence cable, which was formed entirely of another material altogether. It was coated entirely with steel wire outside. There is no doubt that as the St. Lawrence old cable was got up, there were a good many kinks found in it. The reason of that I cannot inform you. The engineers who laid the cable may be able to do so. I know they complained at the time that it was manufactured at another place. It was very interesting certainly, lying as I did off the buoys, in the "Terrible," to observe the Gulf Stream. You could tell to a hair, when there was comparatively little wind, how the Gulf Stream set. If we missed a buoy, we knew that by steering a certain course, after a certain time, we should pick it up again. At the same time whichever way the wind was, it had the greatest possible influence upon the Gulf Stream. There are some gentlemen here, among others Captain Prowse, who being on board the "Medway," had the opportunity of seeing more than I did of the scientific part of picking up the cable.

Mr. GRIFFITH: The only thing that I would say, is with reference to what Captain Heath said about the kinks. There is not the slightest difficulty with regard to them as far as I have been able to observe. The cable takes precisely the same number of turns in it when put on board, as it does the reverse way when it is paid out. His experiment with the tape is very easily explained. In winding up that tape he took care not to put turns in, and when he came to unwind it he then made the turns. But we take care to put the turns in when the cable is coiled, and consequently, they are taken out when the cable is uncoiled. But kinks do occur, only when a whole turn rises from the coil. You can easily imagine that. Instead of the cable running freely out, if a whole turn rises at once, then a kink may occur. But we never had such a thing occur to us during the whole of the expedition, either last year or this. I never saw such a thing, therefore I do not think, as we have had no difficulty in paying out the cable, that we should raise an imaginary difficulty in order to counteract it. Another remark, with regard to the

spiral way in which the wire went round the core. Captain Selwin said it would throw strain on the core.

Captain SELWYN: You misunderstand me. I did not say it would throw strain upon the core, but simply that it would not relieve the core from strain.

Mr. GRIFFITH: The core would take a certain amount of strain no doubt, but it is highly elastic. If you will, however, try to break a piece of that core you will find that the gutta-percha parts before the copper wire, and, therefore, I do not see any danger of fracture of the copper. With regard to the old cable, when it was raised, we never found any part of the core damaged, although the outer serving was very much so. I think the fact of not finding any damage done is a sufficient answer to any imaginary difficulty on that point.

The CHAIRMAN: Should there be any other gentleman present who was on board the "Great Eastern," or the other ships, I am sure the meeting would be glad to hear something from an eye-witness.

Mr. CYRUS FIELD: I had the pleasure of being in the expeditions of 1857-58 and 1865-66, and we were never troubled with any of the kinks which my friend Captain Heath fears. I have witnessed over 6,000 miles of wire being laid across the Atlantic, and we had no trouble of that kind. With regard to other parts of the paper which has been prepared by my particular friend, Captain Moriarty, and whom I came here hoping to meet this evening, I am very sorry to hear him use one expression. He says it takes a considerable time for a cable to get to the bottom. That is very indefinite. Sometimes five minutes may seem a very long time. Some people consider several days a considerable time. However his is a valuable paper, and I wish he had made it a little more correct in that respect. He has made one or two errors in regard to the exclusive right which he says I have obtained, and which I will take the opportunity of communicating to him. But I doubt very much what he says, that if the cable had not been laid this year, it would not have been laid at all. I do not believe that. I do not believe that the English and American people would have allowed this enterprise, after having, in 1865, carried it more than two-thirds across the Atlantic, if we had failed this year, to have remained a dead letter. I fully agree with him that these cables will last. I believe they will last after all who are in this room, are in their graves. I know nothing that should injure them, unless it is where they are very near shore, but where they can be easily repaired. I trust they will long remain to be a blessing to both countries.

Captain HAMILTON: I wish to make one remark with reference to these kinks. I was on board the "Great Eastern." I saw many hundred miles put into the "Great Eastern," a turn put into every coil, and the cable came out of the tank in the same way, and it was paid over the stern of the ship. There never was the slightest sign of kink from beginning to end.

Captain HEATH: My statement has caused so much excitement that I think the cap must fit. It is natural that these gentlemen who have succeeded so splendidly should plead that success in proof of the correctness of all they have done, but I am quite sure, as a practical sailor, that it is utterly impossible to coil or to "flemish" down a rope without putting these turns into it. If you take a rope with both ends fast, and attempt to "flemish" it down, you will find in your hand at last a great lump of kinks. What you did in coiling in your cable, was to get your manufacturer to put one end into a lighter. You coiled this cable carefully and snugly into that lighter, and the end on shore turned round once for every circle that you put into the lighter, otherwise you never could have made that cable lie flat. Any sailor will tell you that you cannot coil down a rope with both ends fast; and if you take the fore-brace as I began by saying, with one end fast, unless the other end is left free to revolve, the best captain of the after-guard in the Royal Navy would never "flemish" down that rope. So I think you never could have "flemished" down your cable and made all snug, without having put in one twist to each coil; indeed, you have told us yourself that you put one twist in for every turn. Then as there were 68,000 turns in one compartment, there were by your own admission 68,000 extra twists. That is your statement, is it not? (Yes.) You have put in, by your own admission, 68,000 extra twists into that rope. You then take one end and tie it hard and fast at Valentia, and the other end is hard and fast in the hold

of the ship. Now then, carry your ship off two or three thousand miles, paying out as you go—how can those twists come out? since both ends of the rope are fast. Every twist you put in remains there. I did not say you necessarily made kinks, but it seems that Captain Moriarty thinks that kinks are there.

The Rev. A. Drew: I happen to remember reading when the first Atlantic cable was laid, that this point was anticipated. It was looked upon as rather a ropemaker's question than a sailor's. It was anticipated that kinks would exist. My belief is that Captain Heath is right, that the kinks are there, but what became of them? The ropemaker will tell you that he made the rope originally slack enough to take up all the kinks, and I believe that is the solution of the difficulty.

Mr. Cyrus Field: One word in regard to what the gentleman has said in reference to these kinks.

The Chairman: I thought you said you saw none.

Mr. Cyrus Field: We have perfect evidence of that with regard to a cable eleven miles long, which we laid from Newfoundland. We often had to cross the strait of Northumberland eleven miles to repair the cables, and we underrun it every length without finding a kink. We have laid it down, and have taken it up.

The Chairman: I think we have exhausted the subject of kinks. It only remains for me to ask the meeting to convey through our Secretary, our thanks to Captain Moriarty for his paper. After the elaborate figures we have heard read to-night, which were not always very easy to follow, we must all I think feel how carefully this enterprise has been prepared, how every calculation has been gone into, and how every experiment has been made that could insure its success. You even see that the very buoys answer their purpose to a nicety. These minute details prove how very great has been the care bestowed upon the operation of laying the cable. I am sure that we are all delighted that its success has been so perfect in spite of kinks.

Evening Meeting.

Monday, March 18th, 1867.

Major-General J. T. Boileau, R.E., F.R.S., in the Chair.

NAMES of MEMBERS who joined the Institution between the 4th and 18th March, 1867.

LIFE.

Murray, John George, Lieutenant R.A. 9*l*.

ANNUAL.

Ellis, C. D. C., Maj. 60th Roy. Rifles. 1*l*.
Edmunds, Charles, Rear-Admiral. 1*l*.
Hill, the Rev. Pascoe Grenfell, Chaplain, R.N.
Crawfurd, H. P. R. F., Lieut. H.M. Madras Staff Corps. 1*l*.
Farquharson, M. H., Lieut. R.M.L.I. 1*l*.
Crickmer, Charles James, Lieut. Durham Art. Mil. 1*l*.
Osborne, Eric Willoughby, Ensign 91st Highlanders. 1*l*.
Hotham, Charles F., Captain R.N. 1*l*.

ON THE CONVERSION AND RIFLING OF CAST-IRON ORDNANCE, AND ON CHILLED WHITE IRON PROJECTILES.

By Major William Palliser, Unattached, late 18th Hussars.

I propose to divide my remarks this evening into three headings. I. The rifling of heavy cannon lined with soft wrought-iron coiled barrels. II. The converting and utilizing of our existing cast-iron ordnance. III. Chilled white iron projectiles. The third heading, properly, is not included in the subject announced for this evening, but since it treats of a materiel which has been largely introduced into the Service, I trust a short notice may not be unacceptable to you.

I shall begin by asking you to be good enough to follow me through what in another place have been lately termed some "Autobiographical reminiscences" as I am anxious to show how working at certain subjects, enabled me to observe wants and defects, and how the experience of these wants and defects induced me to turn my attention to supply and remedy them.

I. *Rifling of Heavy Cannon.*

Some time during the year 1853, I happened to hear it stated that the impossibility of rifling heavy cannon was an universally admitted fact. I had previously made many experiments with cross-bows from which I had shot arrows, bolts, bullets, and even small shot, and had thereby acquired a smattering of the flight of projectiles. In consequence of this knowledge, I believed that an elongated shot could be made to fly point foremost from a smooth-bore gun, by causing its

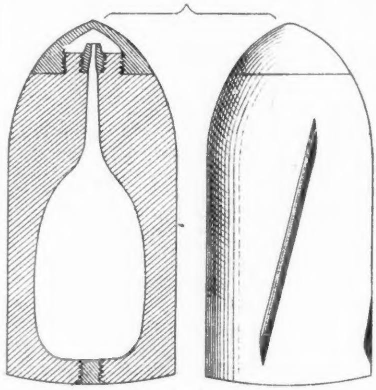
centre of gravity to be placed in front of the centre of the resistance of the atmosphere. This idea has since, and I believe had previously suggested itself to many others. I began by firing a variety of these shells from an ordinary smooth-bore fowling piece, and after a time attained considerable success. These small shells had percussion caps on their points, and invariably exploded against a wooden target at 100 yards distance. I next fired some similar shells out of a cast-iron gun—either a 4-pounder or a 6-pounder—at Dalkey Island from the main land, a distance of about 800 yards. Most of these shells exploded on striking. As, however, they weighed about three times as much as the round shot which the gun was intended to fire, it was believed that their weight might burst the gun, and hence it was always fired with a slow burning fuze, which gave us time to run away and get under cover. I submitted these shells to the Ordnance Select Committee, and they were tried by them during the month of August, 1854, at Shoeburyness, which place was at that time in its infancy. The results were unfavourable, owing to the great amount of windage which I was desired to allow, and which at that time was thought to be necessary. The shells remained point foremost during their flight, but they wabbled very much, and their range was very bad. I have since satisfied myself by further experiments, that great range and accuracy can be obtained with accurately fitting shells with heavy heads, but since there are insuperable objections to mechanically fitting projectiles, and since all difficulties connected with the rifling of cannon, which confers such extraordinary range and accuracy have since been overcome, there would appear to be no object for continuing experiments of this kind, excepting as an interesting scientific enquiry. Fig. 1, Plate VI, illustrates the shells which were fired at Shoeburyness in 1854. Some had helical flanges upon their surface, in order that the pressure of the air upon them might cause the shells to rotate. These flanges, however, confer no advantages. Like all inventors, I was at that time much impressed with the value of my invention, but I perceived that if it were necessary to run under cover every time they were fired, these wonderful projectiles could not be introduced into the service, and hence it was that my attention was turned, at that early period, towards the construction of strong cannon. I had previously watched the manufacture of twist, or, as they have since been called, "coiled" wrought-iron barrels at the forges of Messrs. Truelock and Harris, gunmakers, of Dublin, and had learned from them the great strength derived from this process of manufacture, and I commenced my experiments by casting several small cannon round wrought-iron twist barrels, in the same manner as I still propose to construct heavy guns. By this time, I began to doubt the truth of the statement, that heavy cannon could not be rifled. The letters given below show the precise dates of the experiments:—*

* No. 1.

Gun Manufactory, 233, High Holborn, W.C., Feb. 24, 1866.

SIR,—I beg to hand you the statement of the pieces of barrel I have rifled for you for insertion into cannon, and of the blasting fuze and cartridges, &c., I made

Fig. 1.

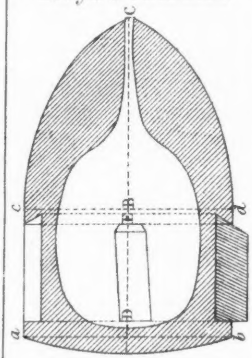


Scale 1/4

Palliser's percussion Shell with Gun Metal false head fired by Ordnance Select Committee August 1854 from Smooth bore 9 Pd^r.

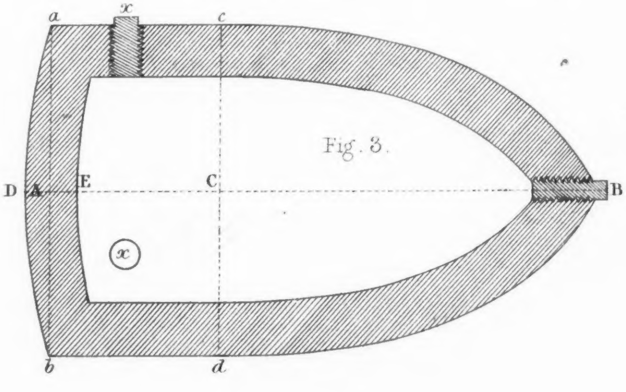
Fig. 2.

Original Sketch received from Lieu^t Palliser in Sept^r 1855 by Andrew Murray Esq. H.M. Dockyard Portsmouth.



A. D.	one inch
D. E.	six inches
A. B.	eight inches
B. C.	twelve inches
a. b.	13 inches
c. d.	13 inches

Fac Simile of original Sketch dated Malta Oct^r 23^rd 1855.



A . B.	20	D . E.	2
A . C.	6	a . b.	12
A . D.	1	c . d.	12

x. x. Round Copper Plugs screwed through the Shell, diameter 1 inch total length 2·5 outside the shell 5 inch

J. Jobbins

Shortly afterwards, the bursting of several 13-inch mortars at the bombardment of Sweaburg turned the attention of many towards constructing stronger guns; and many patents were, in consequence, taken out with that object. I found my small guns so strong that I could not burst them, and now I turned my attention to rifling. Not having sufficient mechanical appliances for rifling guns, I cast several small mortars over sand cores which had the rifling in relief upon their surface, and thus formed the grooves in the mortar. I tried in this manner two or three degrees of rapidity of spiral, and finally made a 6-inch mortar rifled with three grooves. Unfortunately it burst in proof. I,

for you at the time. I am prepared to state that I understood from you that you wanted me to procure and rifle you pieces of gun barrels, for the purpose of inserting in cannon, and that I rifled you three or four pieces in 1854.

I have the honour to be, Sir, your obedient servant,

JOHN W. P. FIELD.

No. 2.

Major Palliser bought of Parker, Field, and Sons.

1854.

July 26. Rifled a barrel twice.
Aug. 1. Powder magazine.
Coil of fuze.
2. Rifled a musket barrel.
1 lb. powder.
11. 1 lb. powder.
17. 1 lb. powder.
Sept. 22. 1 coil blasting fuze.
Rifled a barrel.
Oct. 5. 3 lbs. powder, T.P.
7. 1 do. do.
3 yards blasting fuze.
20. 2 lbs. powder.
5 yards fuze.

The above is a correct extract from our books.

JOHN W. P. FIELD.
Feb. 26, 1866. for Parker, Field, and Sons.

No. 3.

W. Palliser, Esq., to S. H. Seager.

108, St. Martin's-lane, W.C., Oct. 20, 1854.

1854.					
Oct. 20.	To making in oak a large-size model ship's gun-carriage complete......	£2	1	9
31.	To making in oak a large-size model of field-piece carriage..............	5	6	3
		£7	8	0

The above is a correct extract from our books.

July 14, 1866. T. T., for S. H. SEAGER.

No. 4.

15, Gate-street, Lincoln's Inn-fields.

SIR,—On referring to our books, we find that we finished turning a model cannon

however, made another, which was fired by Capt. Sir Thomas Maitland, now the Earl of Lauderdale, on board Her Majesty's ship "Excellent," during the month of August, 1855. The shells employed, had iron flanges cast on them to take into the grooves; these flanges were faced with strips of copper, attached by means of screws. The copper strips were, however, blown off, and could be seen falling into the water. The shooting, however, was indifferent, owing to there having been too much windage. By the kind permission of Admiral Sir Thomas Cochrane, Port Admiral, and Admiral Martin, Superintendent of the Dockyard, the mortar was bored up to a 7-inch, and again rifled with three grooves.

The mortar having been re-rifled with a slotting machine, the grooves were of unequal depth and did not follow the curved surface of the bore. The long flanges on the shells would in consequence not load, and I was obliged to shorten them, until they became mere studs of iron. The windage was reduced to about ·05 of an inch, and, so far as could be judged, by firing down a range, covered with water, the practice was very good. One thing was noticed, namely, the tendency of the shells to deflect to the right. The fact of the iron studs on the shells cutting the rifle grooves was observed, and further that the

for you on the 10th November, 1854. The cannon was of cast iron, cast over an internal tube of wrought iron.

We are, Sir, yours faithfully,
CLARK AND CO., Engineers.

Captain Palliser to Clark and Co.

1854.
Nov. 10. To turning and finishing a cannon, painting, &c. £2 2 0

No. 5.

I, Charles Moritz, engineer, do hereby certify that I finished turning this gun for Captain Palliser on or before the 10th of November, 1854, and I can also identify the gun, both by the pattern and by having filled up with lead some blow-holes caused by faulty casting. The gun is made of cast iron, cast over an internal tube of wrought iron.

Signed, 17th day of September, 1863. CHARLES MORITZ.
39, Lamb's Conduit-street, W.C.

Witness, Edmund Taylor, 7, Russell-terrace, N.W.

No. 6.

British Museum, May 26, 1866.

MY DEAR PALLISER,—I have much pleasure in stating that, during the year 1854, you were living with me in my chambers at 13, Gate-street, Lincoln's Inn-fields, and that, during the summer and autumn of that year you were occupied in making small guns, by casting the barrels over a central tube of wrought iron. On one occasion in the beginning of October, I went with you to Erith and witnessed some of your experiments with them. Towards the end of November, 1854, you left me, and went to Dublin.

I am sincerely yours,
WM. S. W. VAUX,
Keeper of the Coins and Medals, British Museum.

Major Palliser, &c.

greatest injury was done at the bottom of the grooves where the shells first began to move.

My first plan to obviate this was to countersink copper strips in the shot, as shown in Fig. 2. I am indebted to Mr. Andrew Murray, Chief Engineer of Portsmouth Dockyard, for the originals of these drawings. I went down to Portsmouth a couple of years ago, and asked him whether he had any record of my experiments, when he produced from a drawer my letters and drawings, which he had carefully docketed and dated; he also took me to a heap of old iron rubbish, from under which he produced the 7-inch rifled mortar itself. This mortar is at present on the gun wharf at Woolwich, in charge of Captain Gordon, C.B., Principal Superintendent of Stores. It is probably the first heavy piece of ordnance ever rifled on the French system.

If any one will take the trouble to look at the grooves of the mortar, he will see how they have been cut by the iron studs of the shells. I should mention that the shells weighed between 60 and 70 lbs., that the mortar was fired with 4 lbs. of powder, and that a few rounds were fired with only 1 lb. in order to watch the flight of the shell. I well remember that their flight was perfectly steady, that the shells appeared like humming-tops as they passed through the air, and further, that their axes always remained tangents to their trajectories, *i.e.*, that the shells descended with their points foremost. I have often since been surprised to see it stated in scientific works that the axis of an elongated rifled shot remains during its entire flight parallel with the axis of the gun from which it was fired.

I sailed for Malta in H.M.S. "Urgent," the day after the last experiment which took place, on Saturday, Sept. 29, 1855.* We had only left Portsmouth some twelve hours when we burst our steam-pipe, and had to put back into Plymouth for repairs. I wrote from that place the following letter to Mr. Murray, which contains my first proposal for an increasing spiral. My letter is only dated Devonport, Monday. But I am able to fix its date as Mr. Murray has written upon it, "Answered Thursday 4th Oct., 1855."

"I send you a drawing of the mortar sent. If you notice the groove you will see there are two pairs of parallel lines, one pair are straight, the others are supposed to represent the turn beginning by degrees and at last getting into the direct deflection; if the latter could be easily done I should prefer it, as the shell would then be turned by degrees, and not receive the full wrench at the first shock of the powder."

My next letter, dated Malta, Oct. 23rd, 1855, contained a more advanced drawing (Fig. 3.) of the studding system. It says,—"I enclose you a small plan of the shell for a 12-inch mortar, which I think

* Since reading this paper, I have received the following letter, which confirms the accuracy of my dates. The postmark on the envelope, Sept. 30, 1855, shows that the mortar was fired Sept. 29th. "H.M.S. Urgent, Sunday. My dear Vaux,—The mortar was quite successful yesterday, and I have applied to the Admiralty for leave to have one made like it, only of 13-inch bore, and Sir Thomas Maitland will recommend its being done. We sail in an hour. . . . Mr. Murray, the Chief Engineer, will undertake the manufacture of it. There is a man waiting to take the letter on shore, so I must say, good-bye. Yours sincerely,—William Palliser."

would be quite large enough, as the conical 12-inch shell would be larger and heavier than the 13-inch now in use."

The following is an extract from a third letter, dated Camp Notre Dame, Malta, Nov. 20, 1855. "I am making a model 2·5 inch diameter of bore, which I shall send home if it answers; the grooves have a terminal turn of ·6 inch in a foot, but the initial turn is so slight as to be barely perceptible; this goes on increasing till it arrives at the maximum turn as above. I am afraid this is badly expressed, but I think you will understand what I mean. You were quite right in supposing those to be plugs of a mixture of copper and brass, to be screwed into the shell to fit in the grooves. I think they will be quite strong enough to stand the violence of the explosion, and also of the friction. I am not quite confident that ·6 inch a-foot will be sufficient turn for the grooves, but I am decidedly of opinion that it will; and it would be a great advantage if it were, for two reasons—first, it diminishes the friction on the grooves, and the less rapidly the shell rotates, the less will be the deflection to the right.* I also think that ·25 of an inch will be sufficient depth for the grooves, as it will weaken the mortar less,† and likewise cause a less deflection to the right. I would on no account have a greater windage than ·05 of an inch, and would like to have it less if they will allow it at Woolwich."

Now a turn of ·6 inch in one foot is the same as a turn of 6 inches in 10 feet. In a 12-inch rifled gun this amounts to about one turn in 63 diameters. You will then observe that I proposed a 12-inch rifle with ·05 inch windage, a spiral of 1 turn in about 63 diameters, and gun-metal studs screwed into the shells to take the rifling. I find from Capt. Noble's Report, in 1865, on Ballistic Experiments, page 9, on the initial velocity of projectiles fired from the 600-pounder muzzle-loading rifled gun, that the windage for cast-iron shot is ·05 inch, for steel shell it is ·06 inch, and that the spiral is 1 turn in 65 calibres. The windage at present adopted in our service is ·05 of an inch over the studs and ·08 over the body of the shot, and I think the latter might with advantage be reduced to ·05.

I am still of opinion, notwithstanding all the experience that has since been obtained in rifling, that I was right as regards windage, spiral, material of studs, and their mode of attachment as then proposed for a heavy rifled mortar, designed to fire shells of about two diameters in length and containing large bursting charges, and I propose shortly to bring forward such a mortar for trial. I am anxious, however, that it should be well understood that I do not intend by this statement to interfere with the independent enquiries of others who may have been led to somewhat similar conclusions, especially those of Sir William Armstrong, to whose gun I have alluded, and who has so liberally assisted me of late in constructing guns and carrying out my experiments. My object in making this detailed statement has been to show that upwards of twelve years ago I had worked hard

* This sentence shows that we had noticed the tendency of the rifled mortar shell to deflect to the right.—W.P.

† The grooves in my first mortar were deep. It burst right along the grooves, and thus showed me that deep grooves were a source of weakness.—W.P.

at this subject, and to enable you to judge of the progress I had made in it at that time.

On my return to England in 1856, I resumed my experiments by making and rifling several small cannon lined with coiled barrels, and firing copper-studded shot from them; and in January, 1857, I again submitted this system of rifling to the Ordnance Select Committee. Owing to circumstances connected with my profession I had neither time nor opportunity thoroughly to go into the subject until the year 1862, when, owing to a severe kick which I received from a horse, I was placed for several months on the dismounted list, and was thus able to devote my leisure time to making a more extended series of experiments, as well as to compiling a treatise on compound ordnance, which I submitted to the Ordnance Select Committee on or about the 12th November, 1862.

In that treatise I proposed the addition of a small leading stud, in order to combine the advantages of an increasing spiral with those of a double bearing. The theory of the advantages conferred by the accelerated spiral is thus described in page 54:—

"First. If the accelerations of a projectile, from its first starting from a state of rest until it left the muzzle of a gun, were uniform, *i.e.* if equal increments of velocity were added in equal times, then would a straight line inclined at the required angle be the form of groove that would by a uniform pressure gradually accumulate the angular velocity in the shot during its passage out of the gun. For since the increments of velocity in the shot were assumed equal for equal times, the pressure that produced them would always be uniform; and since the pressure on the driving edges of the rifling is always proportional to the pressure that moves the shot, it will be uniform also.

"Secondly. If the velocity with which the shot moved were uniform, *i.e.* if it started off at once with a finite velocity which is not increased during its passage out of the gun, then it will be evident by the former reasoning that the required angular velocity would be uniformly accumulated by a parabolic curve.* .

"Thirdly. If, however, as is actually the case, the projectile suddenly starts off with a high initial velocity, and that this velocity is further accelerated during the passage of the shot out of the gun, it will be seen that the rifling that will cause a constant tangential pressure will be shown by a curve lying between the straight line and the parabola."†

"The spiral which I propose, is such, that were the bore of the gun laid out flat, the grooves would become arcs of a circle, whose radius would be determined by the length of the bore of the gun, and the final angular velocity required.

"The arc will, therefore, be constructed in the following manner:

"Given A B (‡Fig. 7, Plate viii), the length of the barrel to be rifled, and α the final angle of the rifling, at B draw B O perpendicular to A B

* Mr. Twisden pointed out to me that the curve would be a parabola.—W.P.

† The direct, or unaccelerated rifling, causes too great a wrench when the shot first starts, and the parabolic curve does the same when the shot approaches the muzzle. —W.P.

‡ Fig. 9, Plate v in treatise.—Ed.

and make B O of such length that $\frac{A\,B}{B\,O} = \tan \alpha$. With O as centre and O B radius describe the arc B D D'. then B D will be the required curve since a tangent to the arc at D will make an angle with A B $= \alpha$; should it be desired that for a short distance towards the muzzle the spiral should not increase, the tangent D T will give the straight line."

The object of the "differential studding," as Mr. John Penn has aptly termed them, is described at page 75 of the same treatise.

"I at first experienced some difficulty in combining a shot with a double bearing and an accelerated spiral; for since the angle at which the grooves are inclined is continually increasing, we should have the gun trying to turn the fore part of a rigid shot faster than the hinder part, which would be impossible.

"This object is attained (*Fig. 6, Plate vii) by making the rear stud larger than the front one; thus at starting the three studs in rear do all the work of turning the shot, since E E' is the driving edge of the groove when the shot first commences to move. This work, however, is inconsiderable, as the angle of the turn at first is zero. As, however, the shot travels up the bore, the friction will wear down the rear studs, and the assistance of those in front will thereby be gradually called in.

"For a short distance towards the muzzle the rifling will become straight lines, at which place, since the common tangents to the bearing points on the circumferences of the studs become parallel to the driving edges of the grooves, the work will be equally distributed amongst all the studs, and thus the shot, prior to leaving the gun, will be steadied at six points—three in front and three in rear of its centre of gravity. The rear studs will be made large enough to fill the grooves; the size of the front studs will be determined thus: draw A A' a tangent to the larger stud at C, making an angle A' A H equal to the final angle of the rifling. From O, the centre of the rear stud, draw O O', making angle O' O H equal half the angle A' A H. It will readily be seen that a circle described with any point O', along O O', as centre, and the perpendicular O'P let fall upon B B' as radius, will touch D D', and that the shot will freely enter the gun, and that the bearing edges of the studs will all press equally on the driving edges of the grooves as the shot approaches the muzzle."

In this pamphlet I proposed a gun rifled with 3 grooves and an accelerated spiral of which the terminal pitch should be an angle of 5°, *i.e.*, 1 turn in 37 calibres. I fixed upon this ultimate pitch as it was the one which had been adopted by Sir William Armstrong, for the turn of the uniform rifling of his 7-inch breech-loading gun.

Some time after this, the Ordnance Select Committee rifled a gun with three grooves and an accelerated spiral to fire a projectile which had been adopted in the French service, namely a shell with single studs round the centre of gravity (see Fig. 4, Plate vii). This gun was included in the competitive trial which commenced on the 28th June, 1864. Some one, I don't know who, proposed putting in a second small stud in rear (see Fig. 5, Plate vii). This was but a bad imitation of my previous proposal.

* Fig. 3, Plate v in treatise.—ED.

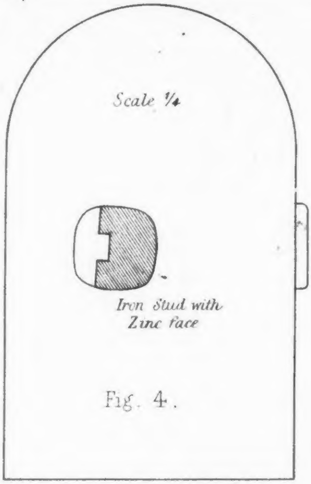


Fig. 4.

French Shot or first supply fired from three grooved Woolwich Rifled Gun.

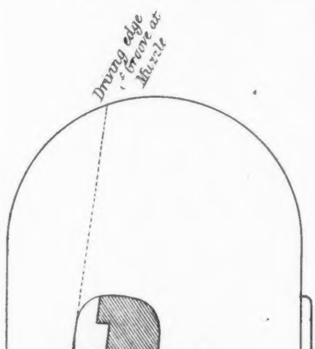


Fig. 5.

Shot of the 2nd supply fired from 3 grooved Woolwich rifled Gun with small Stud attached in the wrong place.

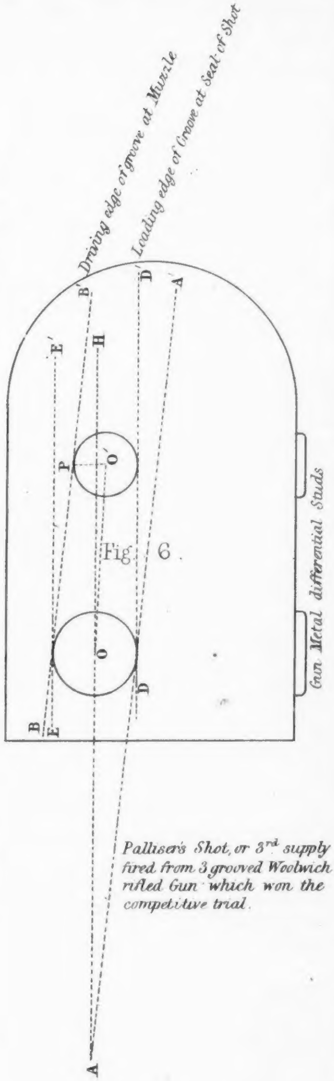


Fig. 6.

Palliser's Shot, or 3rd supply fired from 3 grooved Woolwich rifled Gun which won the competitive trial.

J. Jobbins

Fig 10.

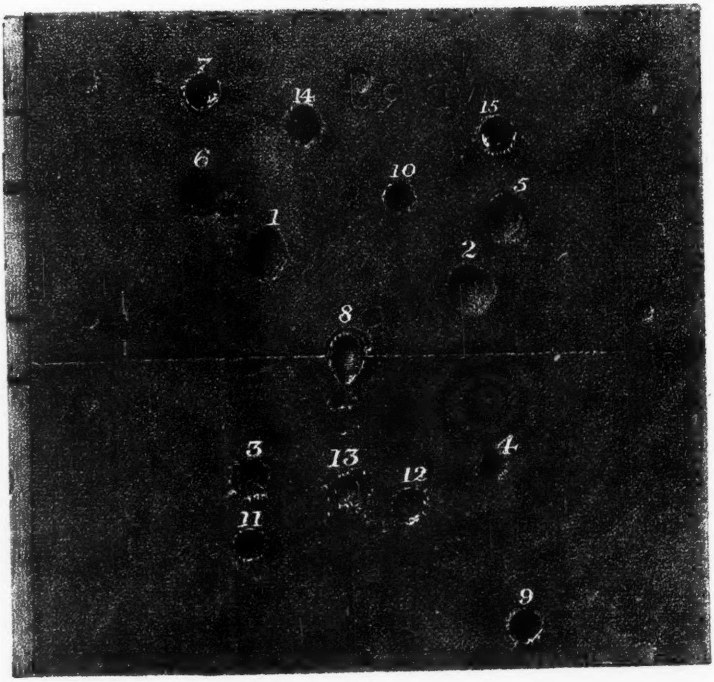


Fig. 8.

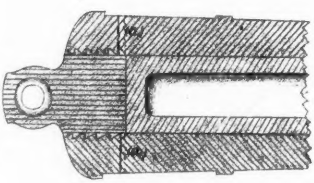


Fig. 9.

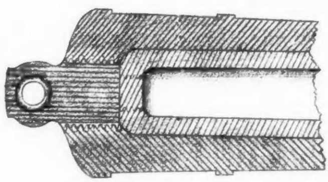
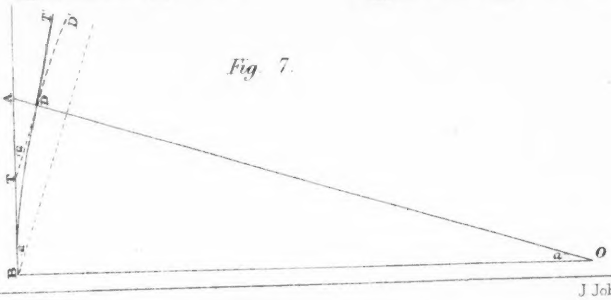


Fig. 7.



J Jobbins

The Ordnance Select Committee, however, tried both methods of studding from an Armstrong muzzle-loading gun, which for the sake of distinction was termed the "French" gun. Both methods of studding turned out failures, the zinc studs which no doubt may have answered well in the French cast-iron guns, fired with low charges and weak powder, were blown off or destroyed by the shock of the large charges of our strong English powder, and the gun made the worst practice of the three remaining in the competition.

I was now invited by the Ordnance Select Committee to send in the shot I had proposed in my pamphlet of November, 1862, and which, as I have shown, were shot perfected from my original proposal of 1855. As soon as these shot (Fig. 6, Plate vii) were tried, the gun came out by itself and beat all the others. I shall now denote this gun as No. 1. The relative accuracy of the three guns which remained towards the end of the contest is given as follows, in a Report published for the House of Commons, July 4, 1865.

No. 1.	Mean difference of range	27·2	yards.
	Mean reduced deflection	3·6	,,
No. 2.	Mean difference of range	38·3	,,
	Mean reduced deflection	7·4	,,
No. 3.	Mean difference of range	47·6	,,
	Mean reduced deflection	6·7	,,

Their respective parallelograms of error would be, No. 1, 78 square yards; No. 2, 283 square yards; No. 3, 319 square yards. That is to say that the shooting of No. 1 gun, firing shot studded on my plan, was about three or four times as good as the shooting of the other two guns. The practice of No. 1 gun was very good, when it is remembered that the 7-inch shot were short, since they weighed only 100 lbs. and were solid, and that the windage amounted to 8-hundredths of an inch. This system of rifling has since been experimentally worked out and perfected by the Ordnance Select Committee, and to accomplish this end many long and carefully conducted experiments have been made by them. The system, as now perfected by them, has been adopted into the service for the rifling of our heavy muzzle-loading guns, and by direction of Earl de Grey, late Secretary of State for War, has been denominated the "Woolwich" system of rifling.

II.—*Converting and Utilizing Cast-Iron Ordnance.*

I propose to utilize existing cast-iron guns in two ways. First, to bore up and line with a coiled wrought-iron rifled barrel those cast-iron guns whose size and pattern will admit of their conversion into rifled guns suited to the requirements of modern warfare.—Secondly. To melt up and recast the others—round similar wrought-iron barrels—into rifled compound guns of the required dimensions.

Having, as previously stated, made up my mind, during the year 1862, once more to resume my enquiries, I determined to place the subject in such a manner before the authorities as would ensure a trial, viz., to submit my plans in a finished state, to accompany them with the

results and specimens of previous experiments as well as with a theoretical treatise corroborated by the calculations of eminent mathematicians, and to produce a rifled gun which had been fired in public, and which could be fired again. I accordingly sought and obtained friendly assistance from Andrew Hart, LL.D., Senior Fellow of Trinity College, Dublin; and also from the Rev. John Twisden, M.A., of Trinity College, Cambridge, and Professor of Mathematics at the Staff College. The calculations independently contributed by those gentlemen to my Treatise on Compound Ordnance will be found at the end of this paper, and it will be interesting to observe how exactly they agree with each other in their conclusions and how exactly those conclusions agree with the practical results of the experiments which will be herein detailed.

I commenced by making a gun similar to the 10-inch cast-iron shell gun of the service—to about $\frac{1}{4}$ scale—and lining it with a coiled wrought-iron barrel. This gun I tested to destruction with increasing charges. I regret that the record of its endurance test, which was very severe, has been lost. The barrel which was rifled had been made in two lengths which were screwed together. The object of this arrangement was to stop the rifling at such a point as to leave a smooth-bore chamber. The first intimation of failure was the separation of the barrel at the joint, and subsequently the casing was broken off. I derived much encouragement by finding that the barrel itself did not burst, Fig. 12, Plate ix. Next, in order to demonstrate the necessity for employing a barrel of *coiled* wrought iron, I made the gun, shewn in Fig. 11. The cast-iron portion of that gun was run round a barrel formed out of a solid bar of the best wrought iron, and bored into a tube. This gun burst at the second round with a charge of only 4 oz. of powder and a shot of $2\frac{1}{2}$ lbs. weight.

I may here mention that subsequent to this experiment a large number of wrought-iron guns were lined with solid-forged tubes. This was done on account of the difficulties at that time experienced, in making sound coiled barrels. The flaws or bad welds in coiled barrels always run circumferentially and although they are no doubt eyesores they are in reality of but little importance. Further the iron used at that time in the manufacture of coiled tubes was of too hard and steely a nature to weld well. This hard iron was deemed necessary in order to resist the crushing force of the powder. Such, however, is not the case, and it is found that soft ductile iron, which I proposed* for making coiled tubes, welds very perfectly, and further that it is set up and rendered dense by a couple of heavy proof rounds. The effect of these rounds is, slightly to bulge the bore at the seat of the charge, and I, therefore, submit my large guns to a severe "setting out" proof before they are fine-bored. The finishing cut thus removes the bulge, and it has now been proved that no further bulge is caused by firing service charges. It has since been conclusively proved that all solid forged barrels are liable to split, and in consequence, it has

* I should wish the innermost tube to be constructed of the softest and most ductile wrought iron, such as Bradley (L.) Charcoal Iron.—Treatise on Compound Ordnance, Jan., 1863, page 17.—W.P.

Fig. 11

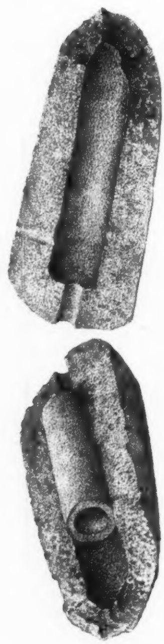


Fig. 12.

Fig. 13.

Pl. IX

Fig. 13.

Fig. 14.

Fig. 15.

Fig. 16.

Fig. 17.

Fig. 18.

Fig. 19.

Fig. 20.

Fig. 21.

J. Jobbins

been found necessary to bore them all out and to replace them with coiled tubes.

After making and destroying several other guns, I made a rifled gun $1\frac{1}{2}$ inches in bore, which I fired during the month of October, 1862, down the range at the Pigeon House Fort, Dublin, in the presence of Colonel Blane, A.A.G., and Colonel Phillpotts, commanding the Artillery. I then took my guns and models over to the Ordnance Select Committee, and at the same time submitted my Treatise on Compound Ordnance. The Committee at once decided upon giving my system a trial and placed a 68-pounder cast-iron gun of 95 cwt. at my disposal.

They decided that I should be allowed to convert their gun, which was of 8 inches calibre, into a 9-inch smooth-bore lined gun. Before doing this I made the following inductive experiment, for I had not previously constructed a similarly proportioned gun. I made three models of the 68-pounder 8-inch gun, accurately to the scale of 1-8th; one of these was a plain cast-iron model of the gun itself; the second was made of a forging of the best wrought iron but not coiled; the third was made of cast iron and lined with a coiled barrel 1-8th of an inch thick, it was 1-inch in bore; and thus represented, to scale 1-8th, the 68-pounder gun converted by means of a barrel two inches thick into a 9-inch gun. The programme I laid down for each gun was as follows:—

Round.	Charge oz.	Weight of Cylinder in oz.
1	1	12
1	1	24
1	1	36
1	1	48

Then repeat the programme with a charge of 2 oz.; then with 3 oz.; and lastly with 4 oz. The cast-iron gun burst at the 4th round, viz:—1 oz. of powder and a cylinder of 48 oz. The wrought-iron and the lined gun each completed the test. They were then submitted to the following test, viz.:—They were loaded with 1 oz. of powder and an iron bar which accurately fitted the bore and projected beyond the muzzle of the gun. The breech of the gun was placed against a cylinder of about 1,000 lbs. weight, and a cylinder of 200 lbs. weight was laid against the end of the bar which projected; the wrought-iron gun burst at the first round, but the cast lined iron actually blew the cylinder of 200 lbs. weight right against the opposite side of tbe cell. The gun was then loaded with 2 oz. of powder and the test repeated, when it burst.

The reason for my giving the details of an experiment made on a scale so small that some consider it to be, in consequence, worthless, is, that I attach the greatest impertance to such inductive experiments in which care is taken that all conditions be strictly proportional and all sources of error eliminated, and I think that one reason why experiments made on a small scale, carry so little weight is, because apparently trifling elements are apt to be overlooked and conditions allowed to creep in which vitiate the comparisons. In order to convert the 68-

pounder into a 9-inch gun, it became necessary to bore it up until a ring of cast iron only one inch thick was left near the muzzle, and some people thought that the "jar" or "vibration," or "concussion," or some such indefinite cause would break the cast iron at that place. But since the metal at the muzzle of the model gun had been only 1-8th of an inch thick, and as nothing of the kind had occurred, I carried out my proposal.

In the early part of the year 1863, the 68-pounder 9-inch gun was completed in the Royal Gun Factories, and it passed through its "test to destruction" without bursting. This test consisted of 100 rounds of 16 lbs. of powder and cylinders increasing every 10 rounds, by the weight of one sixty-eight pound shot. Thus the first ten projectiles weighed each 68lbs.; the second ten 136lbs.; the third ten 204 lbs., and so on, the last ten weighing each 680 lbs. The recoil was very violent, and smashed several carriages. The gun was then suspended in iron slings, these were also broken, and the gun flung itself out of them. It pitched muzzle foremost on the ground which was paved with large stones. The muzzle uprooted some of these and burried itself to a depth of about two feet in the earth, but suffered no injury whatever. When the test was completed, it was found that the lining in the gun had been fissured, or, as it is technically termed "guttered" in front of the seat of the powder; these fissures were upwards of half an inch in depth, and were caused in the following manner. A difference of 15-hundredths of an inch had been allowed between the diameter of the cylinders and that of the bore of the gun. When loaded, the cylinders lay on the lower part of the bore and thus left a lunated space between its upper surface and that of the bore of the gun. When the gun was fired, the heavy cylinders at first moved slowly off, and thus the inflamed gas rushed through this vacant space and eat away the iron of the tube immediately in front of the charge. In this state the gun was then put through a second test with 32 lbs. of powder, and in consequence of the additional 16 lbs lying under the injured—I may say destroyed portion of the barrel—the gun burst at the 7th round with a shot of 200 lbs. weight and 32 lbs. of powder.

The next gun, to which I shall allude, was a 68-pounder, converted into a 7-inch rifled gun by means of two tubes one within the other; the object being that when the inner tube had been destroyed by continuous firing, there might still be a gun to fall back on, which was lined with a stout coiled wrought-iron barrel. For if the inner barrel 1 inch thick were to split, we should still have a lined 9-inch smoothbore gun left, and the great strength of such a gun has just been shown.

The following is extracted from the Report of the Ordnance Select Committee upon its trial.

"Between the 8th October, 1864, and the 16th January, 1865, the gun went through 800 rounds, viz., 20 rounds of 12 lbs., 40 rounds of 20 lbs., and 740 rounds of 16 lbs., and projectiles of 100 lbs. weight. At the 750th round, a crack was observed passing from the bottom of the bore in the direction of the muzzle about 1 inch to the right of the vent, and in the prolongation of the upper groove of rifling; 50 rounds

were then fired without causing much apparent change in the character of the crack, completing in all 800 rounds.

"The A tube is split longitudinally and apparently through, in all about 33 inches in length.

"They (the Committee) are of opinion that the result of the trial is more favourable to the system than if the gun had fired a larger number of rounds and then burst with violence without giving warning. No rifled gun has as yet fired so many rounds with such large charges, and the accident which has led to the suspension of the experiment might to all appearance as well occur to the inner tube of any wrought iron gun. It is probable that the split was first caused by the groove becoming guttered, and that this guttering was accelerated by the use of the round-bottomed shells, a form which would favour the entry of the gas into the grooves before the shells had sensibly moved."

Now the truth of the conjecture of the Committee has since received abundant proof by the splitting of the barrels of several wrought-iron guns. The first instance being that of the steel barrel in the wrought-iron gun rifled on a similar plan, viz., the French system which split about six months afterwards.

I think that the importance of the performance of my gun in having fired 50 rounds with a split 33 inches long running down the powder chamber and through the inner, or A tube, cannot be too strongly dwelt upon. The A tube was subsequently removed by cutting three grooves quite through it. The three segments thus came away from the B tube, which although blackened and covered with dirt by the powder was ascertained to be quite uninjured. The experiment proved the great value of a thin A tube which prevents a crack from becoming deep, and also the value of a stout B tube to prevent explosive bursting when the inner tube shall have been destroyed by continuous firing. The fact of the 7-inch wrought-iron gun having repeatedly penetrated the Warrior Target with chilled white-iron shot and shell of 115 lbs. weight and only 13 lbs. of powder, and of the 6·3-inch wrought-iron gun having done so also with only 12 lbs. of powder, and an 80lbs. shot of the same kind, has added great importance to this experiment which took place three years ago.

My last experiment of endurance was made with a 32-pounder gun of 58 cwt. This gun was one of 8, which on the proposal of General Lefroy, President of the Select Committee, were converted by Sir W. Armstrong and Co., into 64 and 56-pounder rifled guns on my plans, in order to determine various details in rifling, amount of charge, sighting, etc., previous to their introduction into the service. These guns have all got a hole through the cascable—a plan of Sir William Armstrong—for the purpose of giving intimation of any escape of gas. The guns were all converted by this firm, and I have to acknowledge the friendly and valuable assistance they have afforded me. An escape of gas was observed at proof through the breech of one of these guns, and it was in consequence rejected. I have no doubt, that had I asked to be allowed to remedy this defect, my request would have been granted, but since for the sake of economy some alterations had been made in the manufacture I had previously adopted in the 7-inch gun, I was

anxious to test this gun to destruction, and I therefore withdrew it. Before describing its test I may mention that the service charge for similar guns will probably be between 6½ and 8 lbs. of powder, and common shell of 64 lbs. weight, and that they will have battering charges of probably 14 lbs. of powder. I mention this to enable you to estimate the severity of the test, which was as follows:—2 rounds, 16 lbs. powder and 150 lbs. shot; 10 rounds, 20 lbs., 100 lbs. shot; 5 rounds, 10 lbs., and 5 rounds 16 lbs. of powder and 64 lbs. shell. These shells were purposely burst in the gun; they were filled with 4½ lbs. of powder, their fuzes were taken out, and they were loaded with their fuze holes towards the powder. The flash of the discharge thus entered the shell and burst it in the gun. The effects of these explosions were merely to scratch and scrape the bore in various places but they did not in any way interfere with subsequent facility of loading. Next 10 rounds, 16 lbs. of powder and shot of 64 lbs. rammed down only so far as to leave air spaces between the powder and shot varying from 5 inches to 25 inches in length. These rounds produced no apparent effect, 71 rounds were then fired with cylinders of 50, 100, 150 lbs. weight, and charges 10, 15, 25 and 30 lbs. of powder. At last the gun failed under a charge of 30 lbs., and a cylinder of 150 lbs. weight. The casing cracked but no explosive burst occurred. The gun was then cut open and it was ascertained that the wrought-iron barrel was quite sound although considerably bulged. I may mention that the fore-sight was screwed through the casing and caused a large hole in it. The great length of the 30-lb. cartridge brought the strain under the fore-sight where the metal of the gun was also reduced in thickness, and the casing split through this hole.

In judging of these tests the strength of our English powder should not be lost sight of; for most of the cast-iron guns which were previously rifled, burst with a very limited number of rounds, with charges varying from only 5 to 8 lbs. of powder, and shot of between 50 and 65 lbs. weight. Most of these guns were supplied by the Lowmoor Company; I understand, however, that the Italians fire 17 lbs. of powder, and rifled steel shot of 110 lbs. weight from similarly rifled cast-iron guns of the same calibre which they also obtained from Lowmoor. It is certain that one such charge of our English powder would blow them to pieces. I do not mean to infer that the Italians, French, and Americans could not make as strong and even stronger powder than ours, but the fact is that since they do not possess such strong guns as ours, they simply use a weaker powder which is suited to the capabilities of their ordnance.

The bursting of so many cast-iron rifled guns with such small charges, had naturally caused great distrust in this country to the employment of that material, and I have in consequence had to overcome a very strong prejudice. I am anxious, however, to impress upon this Meeting, 1stly, that the guns which I advocate are not strengthened cast-iron guns and that they should be designated coiled wrought-iron guns encased in cast iron; 2ndly, that the interior or heart of the gun is of vital importance, and that if this be composed of coiled wrought-

iron it matters but little whether the outside be composed of wrought-iron, steel, or cast iron.

I shall trouble you with only a few words upon the method of constructing new guns, which I think can be done in the cheapest as well as the quickest manner by running the metal round the barrels. It has taken many years and required many careful experiments to enable me to ascertain the various causes of unsoundness to which this mode of manufacture is liable, but I think I may say with confidence that they have now been all completely overcome. This small gun is a specimen of one nature of unsoundness. Here is another in which the tube is quite loose inside the casing. This section of a gun illustrates the fact of these difficulties having been overcome. It is 3 inches in bore and weighs 8 cwt., and is similar in these respects to the 12-pounder field gun of the service. The following is an extract from a letter I received from the Ordnance Select Committee, dated March 3, 1865, relative to its trial:—

"The test of your 3-inch compound gun has been completed, and the "gun remains apparently unaltered. It has fired—

		Cylinder's weight.
"10 rounds, charge $1\frac{1}{2}$ lbs.	.. ..	24 lbs.
"10 " " "	.. ..	48 lbs.
"10 " " "	.. ..	72 lbs.
"20 " " "	.. ..	96 lbs."

This* gun was subsequently bisected and you will notice that the mechanical fit of the cast-iron casing round the wrought-iron barrel is quite perfect.

My latest experiment in connection with this mode of construction has been with a 9-inch muzzle-loading gun rifled on the "Woolwich" system. The gun was made for me by Sir W. Armstrong and Co. The manufacture of the wrought-iron portion was carried out by Mr. George Rendel, and the casting was performed by Captain Noble. The successful issue is in a great measure due to the care and skill with which those gentlemen carried out the operation. The gun was first tested at Woolwich in a smooth-bore state by firing 20 rounds with cylinders of 250 lbs. weight. The first 18 rounds being with 43 lbs., and the last two with 55-lb. charges of powder. It was then sent back to Elswick and rifled on the Woolwich system, and on its return to Woolwich it was again proved with two rounds of 55 lbs. of powder and shot of 250 lbs. weight, and then completed the programme allotted to it, of 87 rounds of 45 lbs. powder and 250 lbs. shot. It so happened that the inner barrel of this gun was very defective, although it is right to state, that it is the only defective barrel out of upwards of a dozen which the Elswick Ordnance Company have made for my guns.

The mode in which a coiled barrel is made is as follows;—a bar of iron is wound round a mandril, and then welded into a tube about 18 inches long; a certain number of these lengths are then welded

* This gun is now in the Museum of the Institution.—ED.

together end to end, and form the barrel. The coils which formed the second length at the breech end of this barrel were imperfectly welded, and in consequence were separated one from another. Still very little apparent change was caused in these flaws by the test to which the gun was subjected, and I submit that the success of a gun, whose barrel was full of flaws in the powder chamber, argues well for the success of similar guns when lined with sound barrels. Further, I think it will be conceded that having made and successfully fired a 9-inch gun with charges of 45 lbs. of powder, I have proved *à fortiori*, that 8-inch and 7-inch guns which fire respectively battering charges of only 30 and 22 lbs. of powder, as well as the great majority of our guns which are of smaller calibres, which fire smaller charges, and which are therefore exposed to lesser strains, can all be constructed upon the same system.

I have since sent the 9-inch gun to the Paris Exhibition, as I am anxious that it should be publicly recorded that the first large rifled gun constructed on a principle which I feel confident will shortly become universal, was made in this country.

There are two ways in which a gun can be burst, viz.,—by the bursting of the barrel or by the end being blown off. I have hitherto treated of the first mode of bursting, and I shall now say a few words on the second. In an ordinary cast-iron gun, the whole longitudinal pressure acts upon the end of the bore; if the bore be 8 inches in diameter, this pressure will, in round numbers, be distributed upon 50 square inches; if, however, the gun be bored up to 13 inches and lined with a barrel $2\frac{1}{2}$ inches thick, the longitudinal pressure will act upon 50 square inches as before, but it will be transferred to a surface of about 130 square inches, and thus the longitudinal strength of the gun becomes more than doubled. In fact every way of regarding the subject shows that the circumferential strength should be applied internally, and that the longitudinal strength should be borne by the outside, and this is precisely the reverse of the principle on which the wrought-iron guns of the service are made.

Care must be taken that there be no sudden alteration of figure, or rather, that the longitudinal strength be not suddenly weakened at any one place. The method at first pursued in the construction of muzzle-loading wrought-iron guns, was to introduce the interior barrel through the breech piece and then screw in the breech plug or cascable behind it. It will be evident that the plug to close the hole, must be larger than the barrel which passed through that hole, and in consequence that the screw thread to receive that plug or cascable had to be cut *out* of the substance of the gun. This arrangement defined a markedly weak place in the breech of the gun. Now, had the breech to sustain only a statical or gradually applied pressure, this diminution of longitudinal strength would not have been of the slightest importance. When, however, the pressure becomes a blow more sudden and more rapidly applied than any which we can conceive a hammer to give, this difference of strength leads to fatal consequences. I shall endeavour to illustrate this fact by applying sudden or impulsive pressure to some screw bolts. Some of these are made in the ordinary manner, *i.e.*, the

screw thread is cut out of the substance of the bolt, others have screw threads in relief upon the shank like the bolts which I proposed for the attachment of armour plates.

[Major Palliser here placed on the table an iron apparatus, in the form of a tripod, with a vertical rod suspended from the apex, up and down which a perforated mass of iron, weighing about a quarter of a cwt., could move freely. He then produced two bolts, and went on to say:—

"This one is an ordinary screw bolt such as is in general use. It "is a bar of iron tapped at the end. It screws into this nut. The "other bolt is tapped with the same screw tap, but it differs from the "first in this respect, that the shank of the bolt is reduced until the "thread is left in relief upon it. Now, if these two screw bolts were "subjected to a gradually applied pressure, the result would be, that "the first bolt would bear a greater weight than the other, inasmuch "as the sectional area of the screwed part is somewhat larger than "the sectional area of the reduced part. I shall try the ordinary bolt, "subjected to a sudden pressure, and if I am not mistaken, the "weight falling upon it will break it off at once at the screw part."

Major Palliser here screwed the bolt on to the lower end of the vertical rod, and allowed the weight to fall upon it suddenly, the result was that the nut was broken off by the shock. A second experiment with the same description of bolt was attended with a similar result. He then tried the second description of bolt, the bolt with the reduced shank, and the effect was very different. Instead of breaking off under the violence of the blow the iron was simply drawn out, and the blow being repeated several times, the shank became so weak that, at last, it broke in two at the thinnest part.]

I submitted the results of similar experiments to the Select Committee in the early part of 1863, and the original method of closing the breeches of the muzzle-loading guns, has in consequence been altered by them, and inner tubes have ever since been inserted into wrought-iron guns from the front.

III. *Chilled White-Iron Projectiles.*

This small target (Plate viii, fig. 10), is the one upon which I made my first experiments with chilled shot. This is the gun from which they were fired, and these are some of the shot themselves. I am afraid I have already detained you too long and shall therefore only say a few words more. The bore of the gun is $\frac{3}{4}$ inch in diameter. The target is composed of a wood backing three inches thick faced with two wrought-iron plates, the upper one $\frac{1}{2}$, the lower one $\frac{3}{4}$ inch thick. The plates are secured with through bolts reduced in their shanks to the lesser diameter of the screwed part. The gun thus represented to scale $\frac{1}{8}$ a sixty-eight pounder converted into a 6 inch rifle gun, and the target represented to the same scale, 24 inches of wood backing faced with plates respectively 4 and 6 inches thick. The experiments were made in Woolwich Arsenal. The projectiles weighed $5\frac{1}{4}$ ounces each.

Date of Firing.	Number of Round. *See* Pl. viii, Fig. 10.	Charge in Ounces.	Thickness of Plate.	Nature of Projectiles.	Result.
1863			Inch		
May 14th	1	1	½	Wrought-iron Shot, faced with steel; conical head	Penetrated Plate only.
"	2	"	½	Wrought-iron Shot, round head	Plate indented.
"	3	1½	¾	Wrought-iron Shot, faced with steel, flat head	Plate indented; mark of steel ring left in Plate. (*See* fig. 15, plate ix.)
"	4	"	¾	Wrought-iron Shot, round head	Plate slightly indented. (*See* fig. 16, plate. ix.)
"	5	"	½	Wrought-iron Shot, round head	Plate indented.
"	6	"	½	Wrought-iron Shot, faced with steel, conical head	Penetrated Plate only.
May 21st	7	1	½	White-iron chilled Shot, conoidal head	Target completely penetrated; the Shot was subsequently dug out of the earth in rear. It was quite unaltered in shape, with exception of the small tail (to receive the lead) being broken off. (*See* figs. 13 and 14, plate ix.)
"	8	"	½ & ¾	White-iron chilled Shot, conoidal head	Struck on edge of thick Plate and glanced; deep indent through Plate.
"	9	"	¾	White-iron chilled Shot, conoidal head	Penetrated Plate only; the head of the Shot, which was subsequently extracted, was perfect. (*See* fig. 17, plate ix.)
"	10	"	½	White-iron chilled Shot, flat head	Penetrated Plate only and broke into pieces.
"	11	1½	¾	White-iron chilled Shot, conoidal head	Penetrated Plate only; the head of the Shot, which was subsequently extracted, was perfect.
May 25th	12	1	¾	Wrought-iron Shot with steel ring in front, flat head.	Indented the Plate the depth of the steel ring. (*See* fig. 15, plate ix.)
"	13	"	¾	White-iron chilled Shot, flat-headed	Shot broke into pieces against the Plate, which it slightly indented.
"	14	"	½	White-iron chilled Shot, flat-headed	Penetrated Plate only.
"	15	"	½	White-iron chilled Shot, conoidal head	Penetrated Target completely; the Shot was subsequently dug out of the earth in rear. It was quite unaltered in shape, with exception of the small tail (to receive the lead) which was broken off. (*See* fig. 18, plate ix.)

In considering the relative effects of the several Projectiles, particular attention is called to the thickness of the Plate which was struck.

The result of this experiment showed me that a penetration at all events equal to that of any steel could be obtained by the use of *conical-headed* chilled shot, could those of the larger sizes be rendered as hard by the chilling process as these small ones had been made. On the 12th and 13th of November, 1863, some ogival-headed chilled shot and shell (for shape of head see Fig. 19, Plate ix) were fired from a 12-pounder breech-loading gun against 2½-inch plates at Shoeburyness. The results, although they surprised others who were present at the experiment, disappointed me, and I at once perceived that the iron which had answered so well in the very small shot, was not suited to those of larger natures. On my return to my regiment at Norwich, I made a series of experiments at Messrs. Riches and Watts' foundry there, until I obtained large shot of precisely the same nature as the small ones, viz., of chilled white iron. I now, however, encountered a serious difficulty. The Ordnance Select Committee having hitherto carried out all the competitive trials of the steel shot of various makers from the 7-inch breech-loading gun of the Service, it was determined to continue the experiments with my shot from the same gun for the purpose of obtaining comparable results. We now found that the chilled white-iron shot cracked and fell into pieces during the process of lead coating, for the sudden and great changes of temperature, necessitated thereby, split the highly tempered mass, as hot water does a glass. We therefore had to institute the most careful experiments in the Royal Laboratory until we got a shot hard enough to penetrate and yet soft enough not to crack in leading.

From the time, however, that I was allowed to fire shot from the muzzle-loading gun, all my difficulties ceased.

Fig. 19, Plate ix, is a copy of a photograph of the head of a 12-pr. chilled white-iron shot, which penetrated a 2½-inch plate and buried itself 2 feet 8 inches in the earth in rear, on the 29th September, 1864. Fig. 20 is a copy of a photograph of a 7-inch shell, which, filled with sand, weighed 115 lbs., and fired at 70 yards distance with a reduced charge of 13 lbs. of powder, completely penetrated "the 'Warrior' target." Fig. 21 is a copy of a photograph of a 9-inch shell, weight 250 lbs., which, with a charge of 23 lbs. of powder, and fired at an angle of 74°, penetrated "the 'Warrior' target" at 200 yards distance, and burst after penetration. The full charges of the 7 and 9-inch rifle guns being respectively 22 lbs. and 43 lbs., the reduced charges of 12 lbs. and 23 lbs. were calculated to represent a distance of upwards of 1½ miles.

The following table shows the relative powers of penetration of various natures of projectiles referred to the "Warrior" target as a standard:—

None of these did penetrate the "Warrior" Target at 200 yards.	10½-inch cast-iron round shot of 150 lbs. fired with 50 lbs. of powder. 9-inch steel round shot of 115 lbs. fired with 25 lbs. of powder. 7-inch steel shell round-headed of 100 lbs. fired with 25 lbs. of powder.

These have penetrated the "Warrior" at 200 yards.	7-inch ogival-headed white-iron chilled shot or shell, fired with any charge from the service battering charge of 22 lbs. down to one of 13 lbs. inclusive. 6·3-inch chilled shot and shell of 80 lbs. fired with 14 lbs., or even $12\frac{1}{2}$ lbs.

In conclusion I would observe that I have no personal interest whatever in penetrating anybody's target, and that so long as my projectiles make deeper indents than those made by any other projectiles, I am perfectly satisfied, so far as I am myself concerned. On public grounds, however, I hold it to be a source of satisfaction that the guns have beaten the targets. I am of opinion that the more destructive weapons of offence become as compared with moveable appliances for affording immediate protection, the greater will be the superiority of those acting on the defence, and *vice versâ.* I believe this theory is quite general, whether applied to a few men armed with that destructive weapon of offence, the Snider breech-loader, defending themselves under cover of a parapet from the attack of superior numbers advancing across the open, or to the regular attack and defence of fortresses,* or to the attack of armour-plated ships upon coast defences.

As the question at present stands, our rifled cannon could easily beat off the attack of an iron-clad fleet upon any of our maritime fortresses, but should protective appliances be able to defy those of offence, iron-clad ships would be enabled, during any temporary absence of our own fleet, to bombard in security, and at their leisure, those dockyards and fortresses, such as Malta, Portsmouth, and Gibraltar, which lie on the sea coast.

Looking to the great wealth of this country, our extended commerce and isolated position, it will, I think, be admitted that any war in which we may have the misfortune hereafter to be engaged, will most probably be a war of defence on our part, and therefore anything that conduces to the efficiency of defensive warfare, will contribute to the security and welfare of this country.

APPENDIX.

ANDREW HART, LL.D., Senior Fellow Trinity College, Dublin, and the Rev. JOHN TWISDEN, M.A., Professor of Mathematics, contributed the following mathematical papers to my Treatise on Compound Ordnance in 1862.

* Colonel Sir William Denison, K.C.B., R. E., in a letter to Colonel Harness, C.B. R.E., "On the influence of rifled cannon, and small arms on the Attack and Defence," (Professional papers of the corps of Royal Engineers, vol. xii, page 4), says "it is needless to press the comparison farther; it appears to me that the balance of advantage preponderates on the side of the defence; that the siege of any given place would take longer time under existing circumstances, than when the arms used were the musket and the 24-pounder, and the loss to the besieger would be greater."—W.P.

Dr. Hart's Investigations.

This formula of Barlow rests on the assumption that the area of each transverse section is unchanged by internal pressure; in other words, that no transverse pressure applied to iron will alter either its density or its length, or, at least, that one of these effects will so far counteract the other as to leave the area of the transverse section unchanged. As a general proposition, this is (to say the least) highly improbable; and although in the particular case of the cylinder it may be nearly true, yet it should not be assumed as an axiom.

The more commonly received theory is, that the extension or compression of iron in any direction is proportional to the force which acts in that direction; and the following calculation, which I made some years ago, is based on that theory. The result, as will be seen, is that the strain on any lamina of the cylinder is to the strain on the external lamina as the sum of the squares of the diameters of the two laminæ to double the square of the diameter of the smaller one.

Let r be the radius of one of the cylindrical laminæ of which the tube is composed, and let dr be its thickness (the length of the tube being unity); also let p be the total internal pressure on this lamina, and $p-dp$ the reaction of the lamina immediately exterior to it. It is obvious that the difference of these two forces tends to stretch the laminæ, and therefore to increase its radius. Let δr be the extension of the radius, and k the modulus of elasticity (or the ratio of the extending force to the extension), and we have

$$\frac{dp}{2\pi\, dr} = k\frac{\delta r}{r} \qquad (1)$$

But since this same lamina is compressed in the direction of its thickness by the two opposing forces p and $p-dp$, its thickness will be reduced by a quantity $d\delta r$ proportional to the compressing force; that is to say,

$$\frac{p}{2\pi\, r} = k\,\frac{d\delta r}{dr} \qquad 2)$$

(I assume that the modulus of the resistance to compression is equal to that of resistance to extension.)

Multiplying these two equations, we get—

$$\frac{p\, dp}{4\pi^2} = k^2\, \delta r.\, d\delta r \qquad (3)$$

Whence, by integration, if R be the radius of the external surface where $p = o$

$$\frac{p^2}{4\pi^2} = k^2\, \{(\delta r)^2 - (\delta R)^2\} \qquad (4)$$

and, eliminating p between equations (2) and (4)—

$$\frac{d\delta r}{\{(\delta r)^2-(\delta R)^2\}^{\frac{1}{2}}}=\frac{dr}{r}$$

whence, by a second integration—

$$\delta r-\sqrt{(\delta r)^2-(\delta R)^2}=cr$$

and, eliminating c by the condition that when $r=R$, $\delta r=\delta R$, we have finally—

$$\frac{\delta r}{\delta R}=\frac{1}{2}\,\frac{r}{R}+\frac{1}{2}\,\frac{R}{r}=\frac{R^2+r^2}{2\,R\,r} \qquad (5)$$

that is to say, the extension of any lamina is to the extension of the external surface as the sum of the squares of the radii to double their product; but the strains are proportional to the extensions divided by the lengths, that is, as the sum of the squares of the radii to double the square of the lesser radius r. Q.E.D.

In this reasoning it is assumed that the normal compression and the tangential extension are each proportional to the force which acts in its direction, and that a compression or extension in one direction does not cause any change of dimensions in a direction perpendicular to the force. This is manifestly false in ductile bodies, and is probably inaccurate even within the limits of elasticity. This error, however, is in the opposite direction to that of Barlow, before alluded to, and it is probable that the true law is intermediate between the two hypotheses, and that so far as they agree in results they may be safely relied on.

Returning to the equations, we find, by elimination between (4) and (5), and supposing r to be the radius of the inner surface of the tube, that the total internal pressure is

$$p=2\pi k\,\frac{R^2-r^2}{R^2+r^2}\,\delta r$$

and as fracture will take place as soon as $\frac{\delta r}{r}$ reaches a certain limit E, it follows that the total internal pressure must be less than

$$2\pi r\,Ek\,\frac{R^2-r^2}{R^2+r^2} \qquad (6)$$

or that the pressure per square inch must be le less than

$$Ek\,\frac{R^2-r^2}{R^2+r^2}$$

where it is also to be observed, that the value of k is probably less than the coefficient of elasticity given in the tables, on account of the error in the hypothesis which I have already mentioned. Differentiating the expression (6), we find the condition that a tube of given external diameter shall sustain the greatest possible amount of internal pressure, viz.—

$$R^4-4R^2r^2-r^4=0$$

whence

$$\frac{r}{R} = (\sqrt{5} - 2)^{\frac{1}{2}} = \cdot 486$$

If one cylinder be inserted into another so as to fit accurately without initial pressure, and if r' be the radius of the surface of contact, and k, k' the moduli of elasticity of the outer and inner cylinders, then it is clear that the state of tension of the inner cylinder will depend on the pressure at the surface of contact, which is $2\pi k \dfrac{R^2 - r'^2}{R^2 + r'^2} \delta r'$; and that it will be unaltered by changing k and R to k' and R', provided that $k' \dfrac{R'^2 - r'^2}{R'^2 + r'^2}$ retains its value; that is to say, if

$$R'^2 = \frac{(k'+k)\ R^2 + (k'-k)\ r'^2}{(k'+k)\ r'^2 + (k'-k)R^2}\, r'^2$$

Accordingly the strength of the compound tube will be the same as of a simple tube composed of the inner material, with an external radius equal to the above value of R', *observing that* $\dfrac{\delta r}{r}$ *must not exceed the limit of elasticity of the inner tube, and that* $\dfrac{\delta r'}{r'}$ *which is similarly determined, must not exceed that of the outer tube.*

Revd. John Twisden's Investigations.

There are two ways in which a gun may be destroyed—viz., by the bursting of the barrel, or by the end being blown off.

I shall in the first place, consider the former of these, reserving for another place my remarks on the latter.

The force by which the shot is propelled and the gun destroyed, if it be destroyed, is the pressure due to the expansion of the gases formed by the ignition of the gunpowder; and this pressure must be of the same nature as that produced by any other gas in a state of compression; but its capacity to destroy the gun will be affected by its sudden application, by the rapid change it undergoes in magnitude, owing to its expansion, and by the shortness of the time during which its action continued.

The first point for investigation is this:—Given a cylinder filled with a fluid and subjected to a pressure of P lbs. per square inch to determine the strain and the enlargement of the radius caused by that pressure at any point of the substance of the cylinder: for the sake of distinctness, we shall consider a portion of the cylinder whose axis is one inch long.*

* In this investigation, it is supposed that, where the gun consists of two concentric cylinders, they fit exactly, but neither exercises any strain on the other, except when it is subjected to internal pressure.

(1) If we suppose the internal radius of the cylinder to be r, and the thickness of its substance to be very small, then it is well known that, if any small portion of it be taken, it is stretched by two opposite tangential forces whose ultimate value T is given by the equation

$$\text{T} = \text{P}r \quad \ldots \ldots \ldots \quad (1)$$

and if δr is the increase of the radius due to P, we have

$$\frac{\delta r}{r} = \frac{\text{P}r}{\text{E}t} \quad \ldots \ldots \quad (2)$$

where E is the modulus of elasticity and t the thickness of the cylinder, or the difference between its external and internal radii.

(2) Now let us suppose the external radius (R) to differ sensibly from the internal radius (r), but the substance of the cylinder to consist of one homogeneous material: conceive the substance to be made up of an indefinitely great number of cylindrical laminæ; let ρ, and $\rho + d\rho$ be the internal and external radii of one of these laminæ, and let π be the pressure per square inch transmitted to its interior surface, then part of π will be transmitted to the next lamina, and part will be employed in stretching the lamina thus increasing its internal radius from ρ to $\rho + \delta\rho$, let $\pi + d\pi$ be the pressure per square inch transmitted to the next lamina. Now $\pi + d\pi$ being spread over a larger area will be equivalent to $(\pi + d\pi)\dfrac{\rho + d\rho}{\rho}$ on the inner surface of the lamina, and therefore the part of π employed in stretching the lamina is

$$\pi - (\pi + d\pi)\frac{\rho + d\rho}{\rho};$$

and therefore the tension of that lamina (by equation 1) is

$$\pi\rho - (\pi + d\pi)(\rho + d\rho)$$

which ultimately equals

$$-(\rho d\pi + \pi d\rho)$$

or

$$-d.\ \pi\rho$$

Hence by equation (2) observing that $t = d\rho$, we have

$$-\frac{d.\ \pi\rho}{\text{E}.\ d\rho} = \frac{\delta\rho}{\rho} \quad \ldots \ldots \quad (3)$$

Now if all the radii of the laminæ were enlarged by the same amount, the external radius would become

$$\rho + d\rho + \delta\rho$$

But if there is any variation in the enlargement of the several radii, it would become

$$\rho + d\rho + \delta\rho + d.\,\delta\rho$$

Now each lamina is compressed by the pressure transmitted to it; consequently

$$d\,.\,\delta\rho = -\,\frac{\pi d\rho}{\text{E}} \quad . \quad . \quad . \quad . \quad (4)$$

By multiplying (3) and (4) we obtain

$$\pi\rho d.\ \pi\rho - \text{E}^2\ \delta\rho d.\ \delta\rho$$

$$\therefore\ (\pi\rho)^2 = \text{E}^2(\delta\rho)^2 + \text{C}$$

but when $\rho = \text{R}$, $\pi = 0$ and $\delta\rho = \delta\,\text{R}$

$$\therefore\ (\pi\rho)^2 = \text{E}^2\Big((\delta\rho)^2 - (\delta\text{R})^2\Big)^2 \quad . \quad . \quad . \quad (5)$$

Eliminate π between (4) and (5), and we obtain

$$\frac{d.\ \delta\rho}{\sqrt{(\delta\rho)^2 - (\delta\text{R})^2}} = \frac{d\rho}{\rho}$$

Integrating, and remembering that when $\rho = \text{R}$, $\delta\rho = \delta\text{R}$, we have*

$$\frac{\delta\rho}{\delta\text{R}} = \frac{1}{2}\Big(\frac{\rho}{\text{R}} + \frac{\text{R}}{\rho}\Big) \quad . \quad . \quad . \quad . \quad (6)$$

Eliminating $\delta\rho$ from (5) and (6), we obtain

$$\pi\rho = \frac{\text{E}\delta\text{R}}{2}\Big(\frac{\text{R}}{\rho} + \frac{\rho}{\text{R}}\Big) \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Now let δr be the extension of the inmost lamina we have from (6)

$$\delta r = \frac{\delta\text{R}}{2} \cdot \Big(\frac{\text{R}}{r} - \frac{r}{\text{R}}\Big) \quad . \quad . \quad . \quad (8)$$

and

$$\text{P}\,r = \frac{\text{E}\delta\text{R}}{2}\Big(\frac{\text{R}}{r} - \frac{r}{\text{R}}\Big)$$

therefore eliminating $\text{R}\delta$, we obtain

$$\text{P} = \frac{\text{E}\delta r}{r}\Big(\frac{\text{R}^2 - r^2}{\text{R}^2 + r^2}\Big) \quad . \quad . \quad . \quad . \quad . \quad (9)$$

(3) The more usual method of treating the question is, to neglect the compression and to consider that the total area of the transverse

* Formula (6) was communicated by A. S. Hart, Esq.

section of the metal undergoes no change; under these circumstances equation (3) remains the same, but instead of equation (4) we obtain

$$\rho d.\delta\rho + d\rho\delta\rho = 0$$

or,

$$d.\rho\ \delta\rho = 0 \quad \ldots\ldots\ldots \quad (10)$$

If we treat these fundamental formulæ in a similar manner, the following equations can be obtained—viz.,

$$r\delta r = \rho\delta\rho = \text{R}\delta\text{R} \quad \ldots\ldots \quad (11)$$

$$\pi\rho = \text{E}r\delta r\left(\frac{1}{\rho} - \frac{1}{\text{R}}\right) \quad \ldots\ldots \quad (12)$$

$$\text{P} = \text{E}\delta r\left(\frac{1}{r} - \frac{1}{\text{R}}\right) \quad \ldots\ldots \quad (13)$$

It will be remarked that the two methods of treating the question (which I shall henceforth speak of as Hart's and Barlow's respectively) lead to different numerical results, but the truth is probably between the two; hence any conclusion to which they both unmistakeably point may be unreservedly accepted.

(4) I shall, in the first instance, apply them to show the serious loss of strength involved in the construction of a gun of homogeneous metal. For this purpose consider the inmost and any other lamina, and suppose the inmost to be stretched to the utmost extent consistent with safety, then if we had

$$\frac{\delta r}{r} = \frac{\delta\rho}{\rho}$$

the second lamina would also be stretched as far as is consistent with safety; if this were the case, all the laminæ would be on the point of yielding at the same instant, and no part would then be unnecessarily strong. But this is very far from being the case, and it will be found from Barlow's formula (11) that

$$\frac{\delta\rho}{\rho} = \frac{r^2}{\rho^2} \cdot \frac{\delta r}{r} \quad \ldots\ldots \quad (14)$$

To obtain a similar formula from Hart's principles. Eliminate δR between (6) and (8), and we obtain

$$\frac{\delta\rho}{\rho} = \frac{r^2}{\rho^2}\,\frac{\text{R}^2+\rho^2}{\text{R}^2+r^2} \cdot \frac{\delta r}{r} \quad \ldots\ldots \quad (15)$$

Take a gun of 6-inch calibre and 10 inches of metal, and consider the laminæ whose respective radii are 3, 4, 5 . . . 13 in.

Then, according to Barlow, if the first is strained as far as it can be with safety, the second will only bear $\frac{9}{16}$ of what it can with safety, and the others in order $\frac{9}{25}$, $\frac{9}{36}$, $\frac{9}{49}$, $\frac{9}{64}$, $\frac{9}{81}$, $\frac{9}{100}$, $\frac{9}{121}$, $\frac{9}{144}$, $\frac{9}{169}$, of what they are capable of bearing. According to Hart, these numbers would

be $\frac{9}{16}(1 + \frac{7}{178})$, $\frac{9}{25}(1 + \frac{16}{178})$, $\frac{9}{36}(1 + \frac{27}{178})$, $\frac{9}{49}(1 + \frac{40}{178})$, $\frac{9}{64}(1 + \frac{55}{178})$, $\frac{9}{81}(1 + \frac{72}{178})$, $\frac{9}{100}(1 + \frac{91}{178})$, $\frac{9}{121}(1 + \frac{112}{178})$, $\frac{9}{144}(1 + \frac{135}{178})$, $\frac{9}{169}(1 + \frac{160}{178})$.

It is plain from both these series of numbers that, when the inmost lamina is stretched as much as is consistent with safety, the lamina which is only two inches farther from the centre is only stretched the $\frac{9}{25}$ or $\frac{9}{25}(1 + \frac{16}{178})$, say about $\frac{2}{5}$th of what it can be stretched with safety, while those near the outside of the gun are not stretched $\frac{1}{10}$th or $\frac{1}{12}$th part as much as they might be stretched with with safety, and all their remaining strength is useless.

It may be remarked that the factor in Hart's formula, $\dfrac{R^2+\rho^2}{R^2+r^2}$ will be but little greater than unity for the laminæ near the bore, and, since ρ must be less than R, must always be less than 2; from hence it will follow that the strain on the successive laminæ is distributed very nearly according to Barlow's formula.

(5) There is another conclusion deducible from Barlow's formula. Let us suppose the bore of the gun to be increased till its radius is x, and suppose the metal removed to be replaced by an incompressible substance which offers no resistance whatever to extension; under these circumstances the force of the explosion is the same as before, and it is now supported by the part of the iron that has been left; and it can be easily proved that the gun will be stronger than it was before, so long as the remaining thickness of the iron is greater than the radius of the bore.

Since the lining transmits the pressure after the manner of a solid, the pressure per square inch on the internal surface of the iron is reduced to $\dfrac{Pr}{x}$, hence from formula (13) we obtain

$$\frac{Pr}{x} = \frac{E\delta x}{x}\left(\frac{1}{x} - \frac{1}{R}\right) \quad . \quad . \quad . \quad . \quad (16)$$

Now the ultimate strength of a gun is measured by the pressure which stretches the inmost lamina to the bursting point; hence if P' and P'' are the ultimate strengths of the two guns, P' will be the value of P from equation (13), when

$$\frac{E\delta r}{r} = T$$

the tenacity, and P'' the value of P from equation (16), for which

$$\frac{E\delta x}{x} = T;$$

hence

$$P' = T.\ \frac{R-r}{R}$$

and

$$\frac{P'' r}{x} = T \frac{R-x}{R}$$

therefore

$$\frac{P'}{P} = \frac{x (R-x)}{r (R-r)}$$

and $P'' > P'$ so long as $Rx - x^2 > Rr - r^2$, or so long as x is less than $R - r$. The value of x for which P'' is the greatest possible, is clearly $\frac{1}{2}$ R. *If it were possible to remove the iron from the interior of the gun above cited, and replace it in the manner proposed, the gun would be as strong with only* 3 *inches of iron as it was with* 10 *inches, and with* 6½ *inches of iron it would be stronger than with* 10 *inches in the proportion of about* 7 : 5, i.e., *according to Barlow's formula. If Hart's formulæ are employed, similar results,* though much more complicated, are obtained—which in the case of the gun cited lead to a nearly identical result, viz., that the gun is stronger with any thickness of metal not less than* 3·2 *inches, and is strongest when its thickness is reduced to* 6·32 *inches.*

(6) The foregoing investigation is based upon a supposition which can never be realised, viz., that the lining of the gun is an incompressible solid, and one that offers no resistance to extension. We will now examine the case in which the gun consists of two cylinders, one fitting the other exactly; we will denote by r' the internal radius of the outer cylinder, by E' and E'' the moduli of elasticity of the inner and outer cylinder respectively, and we will use the same notation in other respects as before.

We will treat this question on Barlow's principle, *i.e.*, we shall have the two fundamental equations.

$$\rho\delta\rho = r\delta r = r'\delta r' = R\delta R$$

and

$$-\frac{d\pi\rho}{d\rho} = E' \frac{r\delta r}{\rho^2}$$

integrating and remembering that

$$\pi = P \text{ when } \rho = r$$

we have

$$Pr - \pi\rho = Er\delta r\left(\frac{1}{r} - \frac{1}{\rho}\right)$$

* If the internal radius of the original gun is increased to x, the gun will be strengthened so long as

$$x (R^2 + r^2) < R \left\{ \sqrt{R^4 + R^2 r^2 - r^4} - Rr \right\}$$

and its strength is greatest when

$$x = 0\cdot485 \text{ R}$$

i.e., according to Hart's principle.

whence, if P' is the pressure per square inch transmitted to the inner surface of the outer cylinder, we have

$$Pr - P'r' = E'r\delta r\left(\frac{1}{r} - \frac{1}{r'}\right)$$

But equation (13) gives us, remembering that $r\delta r' = r'\delta r$

$$P'r' = E''r\delta r\left(\frac{1}{r'} - \frac{1}{R}\right)$$

and therefore

$$Pr = r\delta r\left\{E'\left(\frac{1}{r} - \frac{1}{r'}\right) + E''\left(\frac{1}{r'} - \frac{1}{R}\right)\right\} \qquad (17)$$

or

$$Pr = r'\delta r'\left\{E'\left(\frac{1}{r} - \frac{1}{r'}\right) + E''\left(\frac{1}{r'} - \frac{1}{R}\right)\right\} \qquad (18)$$

NOTE.—Deductions can be made from Hart's principle in a similar manner, but complicated equations are obtained.

This last equation will enable us to institute a direct comparison between the method of strengthening a gun by encasing it in wrought iron, and that which seeks to effect the same object, by lining it with wrought iron; the difference will be most conveniently exhibited by a numerical example.

(7) Take a cast-iron gun of calibre 6 inches and thickness of metal 10 inches, convert it into a gun whose calibre shall be 10 inches; required (1st) the thickness of the wrought-iron casing, (2nd) the thickness of the wrought-iron lining which must be applied, so that under the same internal pressure per square inch it may be as strong as it was before its calibre was enlarged.

For the sake of simplicity, I shall assume that the modulus of elasticity of wrought iron is double that of cast iron, which is, on the average, nearly the case.

(*a*) For the gun of 6 inches calibre we have (13)—

$$P = \frac{E\delta^2}{r}\left(\frac{R-r}{R}\right)$$

where $r = 3$ and $R = 13$; for the second gun we have (since $E'' = 2E' = 2E$) from (17)

$$P = \frac{E\delta r}{r} . r \left\{\left(\frac{1}{r} - \frac{1}{r'}\right) + 2\left(\frac{1}{r'} - \frac{1}{R}\right)\right\}$$

where $r = 5$, $r' = 13$, and R is the external radius; now P and E are the same in both formulæ, and, the inmost lamina being of cast iron, the weakest point in both guns will be equally tried if $\dfrac{\delta r}{r}$ in both are equal. Now on substituting numerical values, the first formula gives

$$P = \frac{E\delta r}{r} \cdot \frac{10}{13}$$

the second gives

$$P = \frac{E\delta r}{r} \cdot 5 \left(\frac{1}{5} + \frac{1}{13} - \frac{2}{R}\right)$$

Hence

$$\frac{10}{13} = 1 + \frac{5}{13} - \frac{10}{R}$$

$$\therefore \frac{10}{R} = \frac{8}{13} \text{ and } R = \frac{130}{8} = 16\tfrac{1}{4}$$

or the wrought-iron casing must be $3\frac{1}{4}$ inches thick.

(*b*) Let us suppose x to denote the external radius of the wrought-iron lining; in this case the condition of equal strength will be that the inmost lamina of cast iron shall be equally strained in both cases: hence, as before, we have for the first gun—

$$P = \frac{E\delta r}{r} \frac{10}{13}$$

and for the second gun—

$$Pr = x\delta x \left\{ E' \left(\frac{1}{r} - \frac{1}{x}\right) + E''\left(\frac{1}{x} - \frac{1}{R}\right)\right\}$$

were $r=5$, $R=13$ and $E'=2E''=2E$

$$\therefore P\,5 = \frac{E\,\delta x}{x} \cdot x^2 \left\{ 2\left(\frac{1}{5} - \frac{1}{x}\right) + \frac{1}{x} - \frac{1}{13}\right\}$$

the condition of equal strength being

$$\frac{\delta r}{r} = \frac{\delta x}{x}$$

Hence we have for the determination of x the equation

$$x^2 \left(\frac{2}{5} - \frac{1}{x} - \frac{1}{13}\right) = \frac{50}{13}$$

$$\therefore x = 5{\cdot}33.$$

Whence the thickness of the iron lining is about $\frac{1}{3}$ of an inch. *A lining $\frac{1}{3}$ of an inch thick on the inside rendering the gun as strong as a casing $3\frac{1}{4}$-inch thick.*

If we solve the above question, supposing a is the internal and R the external radius of the original cast-iron gun, and if we suppose that the bore is to be increased to r, then if T is the thickness of the wrought-iron casing, we shall have

$$T = R\frac{r-a}{r+a} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad 19)$$

and if t is the thickness of the wrought-iron lining, a general value for t can be easily found, but it is complicated; however, in all practical cases we have very approximately*

$$t=(r-a)\frac{r}{3\text{R}-2r} \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

(8) I will now apply these formulæ to another case. Let there be a cast-iron gun whose internal radius is a, and external radius is R, and another gun consisting of a wrought-iron tube, whose internal and external radii are r and r' respectively placed inside a cast-iron gun whose internal and external radii are r' and R, required to compare their ultimate strengths. The ultimate strength of the former is attained when the inmost lamina is stretched by a force equal to its tenacity (T), *i.e.*, when

$$\frac{\text{E}\delta a}{a}=\text{T}$$

Since the tenacity of wrought iron is at least four times that of cast iron, it follows that unless the wrought-iron tube had a thickness of more than half the calibre, the train transmitted on the cast iron would produce rupture before the wrought iron yielded. Now the wrought-iron tubes suggested in the present paper would never attain this thickness: hence the ultimate strength of the second gun will be attained when the inmost lamina of the cast iron is subjected to a strain equal to its tenacity, *i.e.*, when

$$\frac{\text{E}\delta r'}{r'}=\text{T}$$

Hence if P and P$'$ are the internal pressures producing the ultimate strains in the two cases, we have from (13)

$$\text{P}=\text{T}\,\frac{\text{R}-a}{\text{R}} \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

and from (18), remembering that $\text{E}'=2\text{E}''=2\text{E}$

$$\text{P}'=\text{T}\frac{r'^2}{r}\left(\frac{2}{r}-\frac{1}{r'}-\frac{1}{\text{R}}\right)$$

Hence the ratio of their ultimate strengths is

$$\frac{\text{P}'}{\text{P}}=\left(\frac{r'}{r}\right)^2\frac{2\text{R}}{\text{R}-a}-\frac{r'}{r}\cdot\frac{\text{R}+r'}{\text{R}-a} \quad . \quad . \quad . \quad (21)$$

In this formula, since R is greater than r', and r' than r, it is plain that the first term will ordinarily be much larger than the second; *e.g.*, take

* If we use formula (20) in the above example we obtain $t=\frac{10}{28}$, the exact value being 0·33. An inspection of equations (19) and (20) will show that in all practical cases T must be many times greater than t.

the cast-iron gun as before, and replace the inmost inch of metal by wrought iron, then $R = 13$, $r' = 4$, $r = a = 3$, and we shall find

$$P' : P :: 212 : 90;$$

or the mere substitution of one inch of wrought-iron for an inch of cast on the interior considerably more than doubles the strength of the gun.

If we enlarge the bore up to 13 inches, and then line it with wrought iron 1·5 inches thick, so that $R = 13$, $r' = 6{\cdot}5$, $r = 5$, $a = 3$ we shall find $P' = 1{\cdot}86\ P$, *or the gun's strength is nearly doubled, although its calibre is increased from* 6 *inches to* 10 *inches.*

It can be easily shown that, if a cast-iron gun is cased in wrought iron, whose external radius is R', the ratio of the ultimate strength of the gun will be given by the formula

$$\frac{P'}{P} = \frac{R}{R'} \cdot \frac{R' - r}{R - a} + \frac{r}{R'} \cdot \frac{R' - R}{R - a} \qquad . \quad . \quad (22)$$

which shows that scarcely any thickness of wrought-iron casing will materially strengthen the gun; e.g. in the last example suppose the calibre increased to 10 inches, and the casing of wrought iron to be 4 inches thick, so that $R' = 17$, we shall find $P' : P :: 176 : 170$; or that the strength is scarcely increased. In fact if the thickness of the casing is infinitely great, $\frac{P'}{P}$ only equals $\frac{R + r}{R - a}$, which is never much greater than unity.

The CHAIRMAN: In this very interesting paper which you have heard from Major Palliser, there are doubtless many points upon which gentlemen present may wish to offer observations. All I would ask of you is, as the subject is a very wide one involving much detail, that you will be good enough to confine yourselves strictly to the subject of the paper.

Captain HARRISON, R.A.: There are two points in Major Palliser's interesting paper that it strikes me he did not allude to, and which, I think, are very important both as regards his system of guns and his system of projectiles. It is simply as to their relative cost. Major Palliser evidently, I think, proposes to utilise our cast-iron guns. We must take it for granted that his system will be infinitely cheaper than any system we have at present, if it is equally efficacious. But he also intimated that he proposed to make new guns on this principle. Now, I think it is very desirable, if Major Palliser can do so, that he should tell us what is the relative cost both of the conversion of our old cast-iron guns on the system he proposes, and also of making new guns on his principle. I think the same question applies to his very wonderful—I was going to say, discovery—it is hardly a discovery; but the success he has attained with these chilled projectiles, not only with shot but with shell, and the experiments with which, I have had the pleasure of witnessing from the very commencement, has been very great. He spoke of the satisfaction which everybody had at Shoeburyness, except himself, with the 12-lb. projectiles. I saw that experiment myself, and this is the first time I was at all aware that Major Palliser was dissatisfied. I can only say that if he was dissatisfied, I think he is very *exigeant*, because every one else present on the ground on that occasion was perfectly astonished; so much so, that it led at once to further experiments, which have gone on until the successful results that we see at the present time, have been attained. Not only have his projectiles supplanted steel shot, but we absolutely find, with

regard to steel shell which were reported by the Iron-plate Committee as the only projectiles which were of use for attacking iron-clads, but which are of enormous cost even if we could obtain them of uniform quality, that we can now positively say that steel shell are no longer required, and that we shall simply have shell of chilled iron or white iron, whichever you may call it. But I think it is desirable that Major Palliser should tell us the relative cost of these projectiles; because, with the enormous number that will be required for this country, that becomes a very material question. There is only one other point which occurs to me; it is, that when Major Palliser began to show us that very striking experiment, with regard to the relative strength of those screw bolts, the result was so successful that it drove the thing out of his head, and he did not finish his explanation. Having also had the advantage of seeing the very earliest experiments with those bolts, which were originally submitted by Major Palliser to the Iron-plate Committee, and his earliest letter on the subject as recorded in the reports, I think that those bolts have not yet met with the reward that they deserve. For I must say that uninterruptedly they have been successful. I may be wrong, because I am not aware what decision has been come to, but I should be glad to hear that they have been adopted. The one objection that I have heard alleged against them is not a very strong one, which is, that they do not grip in the centre of the ship, that there is that narrow part. I imagine Major Palliser can tell us whether there is any way of obviating that, or whether it is a *bonâ fide* objection. That I think would also be interesting to us; and if he would be kind enough to say whether the system of stopping the breech of these guns is at all acted upon by the experiment which he began to show us.

Mr. Bramwell, C.E.: I do not know whether visitors at this Institution are allowed to speak. I have asked Major Palliser, and he tells me that they are.

The Chairman: Certainly.

Mr. Bramwell: The last speaker has put some questions about the relative cost of the projectiles made of chilled iron, and of those made of steel. I may say that I had occasion to enquire into the subject, and I discovered this very striking fact, that a steel shell involves, in the first instance, a solid forging which requires to be bored out in order to make the shell cavity. It then requires to be turned externally, and have other work done to it. The result is, that the cost of the steel which is bored out, and made into mere waste turning, is as nearly as possible the whole cost of a Palliser shell of the same size and weight. That fact, I think, puts the question of relative cost into a very simple form. As regards the operation of the bolts for fastening the armour-plates, I, as an engineer, was extremely interested in the result that was developed by the use of these bolts. We are aware that in machinery it is requisite to avoid all sudden changes of form; and Engineers who know their business, take care to specify that there shall not be any small collar, or other abrupt change of form in any piece of iron exposed to motion or vibration; because, if there be, it is a mere question of time as to fracture ensuing. Ensue it must, if there be an abrupt change of form. I have had very many marked cases come before me in my experience. One of the strongest, probably, was the case of a fly-wheel shaft, eighteen inches in diameter, which after working twelve years, broke from no other cause than from having a sudden projection upon it of half an inch on each side. That which has been known to Engineers as a bare fact in the construction of shaft and similar machinery, Major Palliser has certainly applied in the most successful manner to these bolts. When you come to consider the reason of it, I think it is obvious; now that we have been told by Major Palliser how to do it, we may find out the theory. The theory of it, I presume, is this: that when the target is struck, there is a certain amount of momentum which has to be consumed in some way. It may be consumed in elongating a bolt which has two different sections, and where the change from the large to the small section may be very abrupt. The fact is when you pass from the small to the large (I can hardly explain it without a diagram, but I will not be at the pains of doing that) the small section, where it has to endeavour to stretch the large, has the whole force of the shot accumulating at that one point; whereas if you have the shank of the bolt equally small with the bottom, the thread of the bolt is equally capable of extension; and

thereby, the extension of the bolt takes place equally throughout, and the momentum of the shot is consumed in that extension. In fact we saw that when the tripod was put on the table, the elasticity of the table did that which is done by the bolt itself. I think that is the theory of that which Major Palliser has so well brought before us. As regards the projectiles themselves, I was extremely interested with this: viz., that when those projectiles were fired, and broke up as they did in portions, although the points frequently passed through and were entirely uninjured, as they are now, if you picked up the portions that passed through, and laid your hand upon the fractured metal, not upon the outside which had been in the neighbourhood of the target, but on the interior portion, it was barely warm; but if you picked up pieces of a steel shell that had failed to pass through and had broken up, they would burn your hand. The conclusion is obvious, that in the chilled shell there has been no work consumed in altering the form of the shell; but the whole work has been consumed in destroying the target. In the steel shell, the form of the shell had been altered; you could detect it by the eye and by measurement, but even could you not have done so, the test of warmth told you that there had been a work done in the shell in changing its form, and therefore, the shell could not perform the work which it had to do, namely, to penetrate the target.

Captain R. A. E. SCOTT, R.N.: As nobody else seems inclined to rise, I wish to make a few remarks upon the subject. America is now building a large number of wooden corvettes. The vessels of this country will have to be prepared for all eventualities. I need not indicate the powers whom we are *not* to fear, but whom we are still to be prepared for. Now, it is at present in contemplation to lessen the heavy weighting of plated ships, and it has been more than once put forward, that vessels should carry half the amount of ammunition which is at present put on board, and that the remainder of the ammunition should be taken in transports, and supplied abroad. If that be done there will be a saving in weight; but it would still be of great consequence to have projectiles that will be good at all times. If you take a chilled shot, however good it may be against an armour-plated ship, especially when fired at right angles, that chilled shot will be of little or no use, comparatively, in firing through the side of an unarmoured vessel. What you want is steel shell of *large powder capacity*. These chilled projectiles have not that advantage. The chilled projectiles, thanks to Major Palliser and others who have devoted much attention to the subject, are good in attacking an iron-clad ship. But as I have said before in this hall, with respect to steel, you can temper it, so as to be tough. The thinner steel is, the better you can temper it. I believe, with the same amount of attention, and the same means of knowledge given to our steel manufacture, which Major Palliser has applied to chilled shot, that it would result in a cheap steel shell. I may mention that Mr. Whitworth is at the present time making steel shell which have been very *even* in their results, and are somewhat cheaply made; and much more still may be expected in that direction. To my own mind the experiments that have been made in firing angularly at plates (and we may be tolerably sure that that will be the general way in which we shall engage at sea we shall scarcely ever fire at right angles), have not been satisfactory. I see that they are manufacturing at Woolwich at the present time seven-inch chilled shot. I believe there are no chilled shell of seven inches being manufactured. I see that the chilled shell for the nine-inch gun which is being manufactured only holds 2 lbs. 13 oz. of powder. That is certainly, a very small amount, indeed. You want a very much larger amount; and it is steel alone that will not break up with the thinness of shell necessary to carry that large amount. There is another point:—The chilled shell require studs, or some other means of bearing them. The shell that were sent out to Halifax the other day were certainly not covered over, and the studs were smashed up. I know that they can be put in cases or boxes, but in time of war, when large numbers of shell are carried from one place to another, you will never be able to pack them in that way. That was one of the difficulties of the lead-coated shot before, and it is one of the difficulties that we shall get into with stud shot hereafter. I will now turn to the question of strengthening the cast-iron guns. What we want is not only a gun that will stand its charge on being fired, but a gun that will not break up when it is fired at. There is a very

brittle metal outside, which is exceedingly thin. A strong metal is put inside; and as long as that metal is not extended, so as to rest against the cast iron, your gun is secure. But how do you know when it will not extend so as to bear against the outer cast-iron casing? I am not aware of any way in which you can test it. I believe if the firing were continuous that you would find your gun going; especially, if you left off firing in cold weather, and began again in warm weather. I believe the bursting of guns with the present large charges to be a very serious matter; it might result in the loss of an action. Certainly, we could not afford to take on board ship a gun that might burst when fired, or break up when struck by a hostile shot. There is another point I would mention. As far as we have experience at the present time of coiled tubes, it is exceedingly difficult to weld them together so as to make a complete joint. When fired with high charges, the coils separate. When that gun of Sir William Armstrong, the 300-pounder, which afterwards blew its breech out, was proved, the coils changed in the proof; at each round there was an alteration in the inner tube. We know that there is great difficulty in welding an iron tube so as to make it stand for the interior of a gun, and you cannot put cast-iron outside a steel tube. The only gun that was tried, that was made in that way, burst at the first round. I believe it to be essential for the endurance of our guns that we should have a steel tube inside and the strongest metal we can put on over it. I regard the efforts that are now made at the Gun Factory, at Woolwich, as most satisfactory; and I think great credit is due to the Superintendent and his manager, Mr. Fraser, for the system of manufacture they have introduced, which is undoubtedly at the present time the best known. I do not think I need go particularly into the fact as to the competitive guns. I can only say that I dissent in toto about No. 1, No. 2, and No. 3; I do not wish to go into that. But as to the gaining twist, there is a great mistake usually made. It is said that your shot should start easily, and should then be whirled up. It happens that when you push your shot home, if you have an even twist, that the stud or bearing, or whatever it may be, is resting against the loading edge; and when the powder is acting upon it, it has a little space to move in, whether it is the 800th part of an inch, or whatever it may be, before it touches the bearing side of the groove, and, all danger is over as soon as the shot has moved. Therefore, I do not see the value of that argument as to the gaining twist, and it seems that Major Palliser himself has very great doubts on the matter. I think that great variations have been made in this rifling. When Captain Haig, the Assistant Secretary of the Ordnance Select Committee, assisted Major Palliser in rifling his gun, the rifle twist commenced not where the shot lay, but it was at that point very nearly half the amount of twist at which the rifling terminated. I observe that the Ordnance Select Committee are gradually working back to the even twist, and I have not the slightest doubt they will end there. Despite what Major Palliser has done, and he has shown some admirable experiments this evening, and is entitled to very great credit (nobody who has taken these things in hand but knows what great thought and attention they require, and must give Major Palliser full credit for everything he has done in that respect), I say despite all this, I dissent with respect to strengthening the cast-iron guns; and I dissent also with respect to introducing a chilled projectile without a much fuller trial of steel, which I am sure is the material we shall ultimately come to, just as we shall come to an even twist in rifling.

Captain TYLER, When Major Palliser replies, I hope he will be good enough to tell us something more about the chief points of his projectiles; and whether he has fixed upon any particular shape as being the best. Captain Scott referred to that subject to some extent. And perhaps he (Major Palliser) would go on to say whether he has tried flat-headed shot. I observe that the shot in some of those diagrams have round heads, and not pointed heads, such as those which have gone through the armour-plate. It will be very interesting to know what, in the course of the experiments, are the exact results obtained from different shaped heads to the shot. As I have understood hitherto, these pointed shot will go through the armour-plates more easily when they are fired directly at them; whereas the square-headed or round-headed shot will penetrate more easily than pointed shot when they are fired at an angle towards them. With regard to the bolts that Major Palliser has shown us,

the experiments were exceedingly interesting; and remind me, as they must have reminded Mr. Bramwell, who spoke so well on the subject, of the way in which axles have come to be made in railway engines and railway carriages. It was found when the axles were made with parallel sides that from the want of elasticity they broke rapidly and suddenly near the wheels; but by being made thinner and thinner in the middle, they at last arrived at a point when they were actually stronger than when they were thicker. Very much the same result occurs with these bolts of Major Palliser. By making them thinner at one part he really makes the whole bolt stronger than they would otherwise be. There was one point brought forward by Mr. Bramwell that I could not agree with; and that was with reference to the reason why these chilled shot penetrate so much better than the steel. Mr. Bramwell gave us a very lucid explanation upon that point, and told us how it was that the steel shot was much hotter than the chilled shot after it had struck the target; and he deduced from that fact that the steel shot had done its work in altering its own shape, while the chilled shot, as I understood him, had expended its work in penetrating the target. Now, I apprehend, that steel shot and chilled shot have both the same amount of work to do in penetrating a target. I did not quite understand how it could be, if there be an alteration of shape in the steel shot, why so much heat should be developed. I should like to know whether that has been satisfactorily ascertained from a great number of experiments, or whether there may have been some mistake about one accidental case. It is quite clear that steel shot may be made so very hard that almost no alteration of the shot would occur in striking a target, as is the case with chilled shot. Therefore, if we could get a little more information upon this subject, and perhaps Major Palliser will be able to give it to us, it would be very interesting.

Captain Molony, R.A., Asst. Sup. of Royal Gun Factories: There is one point on which I hope Major Palliser will offer some explanation: that is, how he proposes to give the necessary strength to his compound guns in the longitudinal direction. The effect of the longitudinal strain, he has clearly explained, is to blow the breech off. It is in this direction his guns have always shown great weakness; and it is in this direction that, even in guns altogether of wrought-iron, there is a tendency to weakness. Major Palliser—I do not know that he proposes putting steel into his guns—puts a wrought-iron coiled barrel into a cast-iron casing. Wrought-iron coiled barrels cannot be made with solid ends; consequently the whole longitudinal strain, coming on the inserted cup or plug at the end of the bore, is at present resisted by the cast-iron casing alone. I hope that he will clearly explain how he proposes to strengthen guns in that direction.

Dr. Tyndall, F.R.S.: I am exceedingly reluctant to add to the weight already put upon the shoulders of Major Palliser by adding to the questions proposed to him. I would beg permission to express the great gratification with which I have heard this paper this evening. I think the spirit in which this investigation has been conducted is the true spirit. He has gone on making his footing sure, basing himself on the sure ground of experiment, as he went on, making experiments on a small scale before he attempted experiments on a larger scale. I must say that the course he has pursued is such as will be justified by the experience of every scientific man accustomed to experimental research. Now, I dare say there are many persons present who would very much like to know the precise process by which those wonderful bolts have attained their present tenacity and rigidity. We hear of chilled shot; and I am equally persuaded there are many present who would like to hear from Major Palliser's own lips how this process of chilling is conducted. With reference to the observations that fell from Mr. Bramwell, respecting the amount of heat developed, no doubt when a projectile strikes a target, the total amount of heat developed by the stoppage of a certain amount of motion, is always the same. If the Palliser projectile goes through the target, and if a steel projectile of the same weight and velocity stops short and does not go through, there is a greater amount of heat developed at the target in the case of the steel, than of the Palliser projectile; there is no doubt of that. It depends entirely upon the amount of *vis viva* annihilated. Then the question arises, how is the heat distributed? That depends upon the comparative rigidity of the target and the projectile. If the

statements I have heard to-night are correct, the proportion of the total heat which comes to the share of the projectile is greater in the case of the steel projectile than in the case of the chilled shot. That would prove that the density and rigidity of the chilled projectile is of such a character as to throw the heat more upon the target than upon the projectile. I say that, as far as this goes, it is in favour of the chilled projectile.

Mr. Bramwell: I may say, for the information of the gentleman who asked that last question, in whose views I very much coincide, that I said studiously, if you put your hand upon the interior of the broken-up shot, the interior of the Palliser shot or shell had no heat that was at all harmful to the hand, while the steel shot or shell, owing to the change of shape that had taken place had the heat developed in its interior. Of course, with regard to the exterior of the shot, whether it were chilled shot or steel, its heat would be due to contact with the target.

Mr. Mallet, C.E., F.R.S.: I think there has been a little misconception as to the importance of this question of heat. I happened to have been present myself on the 24th of October last, when the experiments were made to which Mr. Bramwell has alluded, when steel shell was fired along with a number of chilled shot and shell. I felt the fragments of both of them in my own hand. Those of the steel shell were undoubtedly colder—the portions that I took—than those of the chilled shot. But I attribute no importance to that one way or the other. If you attempt to estimate the relative values, the practical values of projectiles by refinements derived from questions of the amount of heat developed, you will go far astray. It is simply preposterous to talk of there being any superiority in the properties of chilled shot over those of steel projectiles on such grounds. It is a question of comparative price and practical utility, of convenience rather, in use as an artillery instrument. If you can make to the end of the chapter chilled shot cheaper than you can make steel, then I say by all means adopt them. My belief is, knowing something practically of the question in other directions, that the time will come, and is not very distant, when cast-steel shot or shell will be made quite as cheap as chilled ones. Steel shell are being made now quite as cheap, as far as my information as to their cost at Woolwich goes, as chilled shell, and they will be found, I believe, decidedly superior. We know pretty well what the co-efficient of cohesion is, and what the co-efficient of rigidity is in both. Now chilled cast-iron, means iron that is soft in the interior and hard on the outside. These shot which were tried on the 24th of October and produced such remarkable effects, and which I entertain a very high opinion of, were not in the proper acceptation of the word chilled shot. They were simply white cast-iron shot, and would have been much the same had they been cast in sand. If you can at the same price make cast-steel shot or shell, you will have a better projectile than any projectile you can make of chilled iron or cast white iron. There have been so many points brought forward to-night that it would be quite out of the question to attempt to refer to them in an orderly manner. But there is one point more which I think Captain Tyler alluded to, that I would remark upon; namely, as to the advantage obtained by the use of pointed projectiles compared with flat-headed projectiles. I was, myself, in company with Mr. Whitworth and several other persons, originally an advocate for flat-headed projectiles. I gave in evidence before the Iron-plate Committee my opinion as to what sort of flat-headed shot would be most likely to penetrate armour-plate. I believe I was entirely wrong. The first man, I believe the first man in Europe who on *à priori* principles showed that that view was wrong was no one in this country, but Professor Haughton, Fellow of Trinity College, University of Dublin, who in a paper published in the proceedings of the Royal Irish Academy, and as resulting from investigations made by him, predicted the fact that a shot having a conical point, or with an average angle of about 70 degrees, which is what has actually been come to at Woolwich, would, on principles that he had already applied to a totally different subject, viz., physical geology, be that which must necessarily penetrate amour-plate with the smallest expense of force. I believe that great merit is due to Major Palliser for having persistently hammered into the ears of the authorities that the hardest and densest and toughest material was that which would be the best for penetrating armour plate.

Dr. TYNDALL: Will you allow me to say one word with regard to the development of heat. Mr. Bramwell has said something about the contact. The heat as developed is entirely measured by the amount of motion stopped. If you take the velocity of the projectile just before it strikes the target, and, supposing it to pass through the target, if you take its velocity immediately after it has passed through, the difference between those two velocities is the measure of the heat produced. It may be a minute, or a second, or part of a second, it is entirely independent of time; it depends upon the amount of motion stopped in passing through the target.

Mr. MALLET: In every case it is divided between the distortion of the shot and the distortion of the target.

The CHAIRMAN: If no other gentleman has any remarks to make, I will now call upon Major Palliser to reply.

Captain TYLER: Just allow me to say, that Dr. Tyndall has given us the proper theory as to the amount of heat that was developed in the case of shot passing through the target. Would he go on to state what occurs when the shot hits the target, and fails to pass through? In that case, the whole momentum of the shot must be turned into heat.

Major PALLISER: Captain Harrison asked me first as to the relative cost of the conversion of existing guns, and of the construction of a new gun of wrought iron. It s impossible for me to say what the relative cost would be, as I have been unable to ascertain the cost of a cast-iron gun converted, or what is the actual cost of a gun made entirely of wrought iron. But this much I will say, that in that 12½-ton gun of mine, there was an interior barrel of wrought iron which weighed 2½ tons, and that the expense of that barrel could not be more than the expense of the same amount of wrought iron in a wrought-iron gun. There remain, however, ten tons of metal—of cast iron which were run round the barrel in about one minute and a half; and I leave it to anybody to form their opinion as to the relative expense of casting a mass of cast iron like that in a couple of minutes—when we have the cast-iron already in our possession not costing us anything—and the cost of making, turning and boring rings, shrinking them one over the other, and thus gradually building up a wrought-iron gun. With regard to the relative cost of the projectiles, I think Mr. Bramwell has answered that. I believe the cost of chilled shot is very little more than the cost of what they call common shell—the ordinary cast-iron shell filled with powder. The fact is that the chilled shot are so cheap, that I believe they are going to use them in practice.*

Mr. MALLET: Can Major Palliser tell us what is the actual cost in pounds sterling per ton for manufacturing, at Woolwich, the so-called chilled shell and shot?

Major PALLISER: I am not able to tell you that now; but I know than when they were first commenced, it was about £18 per ton. But we have got it lower than that now. One great advantage of the chilling process is, we find—at all events we feel pretty sure—that we shall be able to utilize the existing stock of cast-iron shot and shell in the manufacture of chilled shot, and then they will be very much cheaper. But Mr. Mallett has said that casting white-iron shot in sand is not different from casting one of these chilled shot. Now, I beg to say, that the first person who analysed a specimen of chilled shot was Professor Abel, the chemist of the War Department, in 1865. I asked him to analyse some portions of the chilled shot, and he did so, and he found they differed in no way in their component parts from ordinary white cast iron. Then, it occurred to me, if that is the case, there might be no objection to cast white-iron shot in sand. But it was found that the grain of such shot was very open. If that chilled shot were split open, the grain would present a most extraordinary appearance, and it is this intense hardness and closeness of grain which contributes much towards the superiority of the chilled shot.† This closeness of grain is caused by the violent contractions of the

* This question has since been decided in favor of using common shells in ordinary practice.—[ED.]

† NOTE.—Subsequently communicated by Major Palliser. "A 7-inch, white iron shot, cast in sand, weighing 123½ lbs., has been lately fired at the "Warrior" target with a 15 lb. charge, and failed to penetrate it, while a chilled shot of 114 lbs.

exterior portions of the projectile which become suddenly solidified and compress the interior while still in a liquid state. In addition, the chilling process enables you to utilise a great quantity of iron which you otherwise could not. Further, the iron employed is made so intensely hard, that it cannot be turned; and you can cast the iron in an iron mould more accurately than you can cast it in sand, unless you go to the expense of dry loam moulding which is more expensive. Another advantage of the iron mould is the facility it affords for casting the stud holes in the shot. Altogether, the advantages conferred by the chilling process render it far superior to casting shot possessing similar properties in sand. In 1864* I pointed out that the whole discovery in these shot consisted in finding out that that *toughness*, on which Mr. Mallett has laid so great stress, was of no importance; and that all that was required was great hardness—or rather resistance to compression—combined with a form of head which would apply the pressure *gradually* to the shot. I believe that a blow is nothing more nor less than a severe pressure applied for a short time; that to flat-headed shot this pressure is *suddenly* applied, whereas to the conical or ogival-headed shot this pressure is *gradually* applied. If you refer to the Report of the Iron-plate Committee in 1865, and that of the Armstrong and Whitworth Committee, you will find they call attention to the great tendency of steel shot to break up on impact, and to the great care with which it was necessary to temper these shot. In fact, the Committee looked upon toughness as one of the most important elements in the shot. Captain Scott said that these shells had small capacity. I beg to say that shells of large capacity have just as good penetration as shells of small capacity at the direct target. At the inclined target they have not. At the inclined target the solid shot has better penetration than either kind of shell. But as far as firing at a direct target, or anything moderately direct, the shell of large capacity has a penetration equal to either the shell of small capacity or the solid shot. With regard to flat-headed steel shot, one of 250 lbs. was fired from a 9-inch rifled gun with 43 lbs. of powder at 200 yards distance at the 8-inch plated "Warrior" target; it made an indent $4\frac{1}{2}$ inches deep and rebounded from the plate; while a chilled shell of the same weight fired the same day from the same gun with the same charge, went right through the target and burst on the other side. I do not think it is necessary to say much about the liability of converted cast-iron guns to break up under continuous firing, when the first rifled lined gun I made was a 68-pounder converted into a 7-inch rifled gun and it fired 800 rounds without bursting. With regard to the uniform spiral and the increasing spiral, the very fact that Captain Scott has mentioned, that the shot at first hugs the loading edge of the groove and leaves a space of say the 800th part of an inch between the

weight, fired at a similar portion of the target, on the same day, with the same charge, and from the same gun, penetrated the target completely and tore away some supporting beams in rear."—[Ed.]

* *Vide* the following extract from Major Palliser's letter to *Mechanics' Magazine* of 22nd July, 1864:—

"At the time I first heard of Mr. Whitworth's tempered steel shot, I conjectured that the results he obtained were to be attributed as much to the hardness as to the toughness and tenacity of the material he employed. I had previously noticed in some experiments that I had made in casting iron round wrought-iron tubes, that, under certain conditions, the cast-iron was rendered very hard and unyielding where it came into contact with the cold wrought iron. I was aware that cast-iron would resist a far greater crushing force than wrought iron could, and putting these facts together, I thought it highly probable that if I cast an iron shot in a mould that would rapidly carry off the heat by conduction, and if at the same time I made the fore part of the shot of such a form as would convert the sudden shock of impact as much as possible into a uniformly increasing pressure, the brittle nature of the material would not be of much consequence. I accordingly made a model iron-plated target, a small rifled cannon, and cast some shot to prove the truth of my conjecture. At the time I commenced the experiment, I had never heard the expression "casting in chill." As soon as I had satisfied myself by experiment that the theory held good on a small scale, I sent the gun, target, and shot, last May twelve months, to the Ordnance Select Committee."—[Ed.]

surface of the stud and the driving edge of the groove, shows that when the shot first starts, it does not at once come in contact with the grooves. Therefore, the shot must move through a certain space before it meets with the sudden resistance of the inclined groove; and the amount of that sudden resistance would be measured by the initial angle of the rifling. What I mean is this: if that were a rifled groove, and this the flange on the shot (pointing out the different parts), as Captain Scott said, it hugs the loading edge. When fired, he says it does not immediately touch the driving edge. This is quite true. But what is the result. That flange, whose tendency is to go straight on in that direction, suddenly comes in contact with this uniform spiral groove, when the shot has not only received the first shock of the powder, but when a considerable amount of velocity has also been accumulated in it. The stud, or flange, therefore, strikes a violent blow against the groove. This is a matter of little importance with small guns; but when we come to use large projectiles fired with high velocities, you must have an increasing spiral groove. Why I lay particular stress upon this is, that I am an advocate for the coiled barrel, despite what anybody says. I say that the grooves in this soft material will not stand the shock of such a heavy body coming suddenly upon them. If you must have an uniform spiral, you must also have a great number of grooves, and a great number of studs for each groove, in order to distribute the pressure over a large bearing surface. If you want a large gun to fire heavy shot with a moderate number of studs on them, you must have the increasing spiral groove, and I see no objection to it whatever. With regard to the particular form of the head of the shell, it is what is called an ogival head, and is formed by the intersection of two arcs of a circle whose radius equals $1\frac{1}{4}$ diameters of the body of the shell. This form of head so far as our experience has gone, has penetrated better than anything else. With regard to solid shot, the Ordnance Select Committee have sacrificed a small amount of effective power in order to get a longer body and therefore better bearing in the gun, which gives increased accuracy of shooting. They have hitherto struck the head with a radius equal one diameter, but I believe in future they will take one and a quarter diameter. With regard to the seven-inch shells, I believe the reason seven-inch *shot* have been ordered was that the authorities were anxious to obtain at once something which they had found to be good, to adopt it at once, and to trust to experiments for further improvements. Next year, however, we shall have 7-inch shells.

The CHAIRMAN: A gentleman has asked how the shot are made?

Major PALLISER: The shot are made in this way. There is an iron mould which is in the form of the shot, and there are holes made in it in which sand studs are put; the metal is simply poured down through a hole in a sand top or covering—the sand studs in the mould, cast the holes in the shot to receive the gun metal studs, and the shot comes out practically finished, with the exception of putting the studs in and cleaning off any roughness, and painting.

The CHAIRMAN: Will you be kind enough to complete the explanation of the cascable of the gun?

Major PALLISER: If you notice, the breech of a gun is the converse of the screw bolt. A female thread is cut out of the body of the gun, which is precisely the converse of the male thread cut on the screw bolt. Hence, the sudden shock which comes here (pointing to the screw thread in the breech of the gun), gives a tendency to the gun to break here (see Fig. 8, Plate viii). And a gun did actually break off its breech at that place. This fact has been harped upon a great deal; and I think it only fair to state that the powder employed was of an excessively strong and violent character, and such powder has never been used since. It was what is called No. 2 A 4 powder; further the charge was fired at two places at once. That is to say, there was a vent towards the rear end of the charge, and a vent towards the front, and the large powder charge was fired simultaneously at both points by electricity.

The CHAIRMAN: I feel confident that I am expressing the wish of the meeting in returning to Major Palliser our warmest thanks for the very interesting paper which he has read to us. I have no doubt you were all remarkably struck with the successful progress of his experiments, as one after the other, they have by induc-

tion led to their present completeness. The success which has attended the progress of his experiments, is very remarkable as regards the facts connected with his projectiles. And although it may be that for wooden vessels chilled shot is not the particular missile most suited for offensive use, that is not the question which has been under consideration. It is the piercing of armour-plated vessels; and in attempting that, Major Palliser's success appears to have been complete. Many gentlemen have brought their great talents to bear on the subject of artillery, and have given the benefit of their knowledge to improve the offensive and defensive means of our country. Most of them have reaped but a barren reward for the practical issue of their experiments. I trust Major Palliser not only will rejoice in the knowledge, and feel satisfied with the reflection of having done so much, practically, for the good of his country, but that he may also reap what is more unusual, an adequate substantial reward, in the shape of a pecuniary grant from Government.

Evening Meeting.

Monday, April 8th, 1867.

General Sir WILLIAM J. CODRINGTON, G.C.B., in the Chair.

NAMES of MEMBERS who joined the Institution between the 1st and 8th of April.

Freeth, Walter, Esq., Commander-in-Chief's Office. 1*l*.
Busk, Hans, Captain, 1st Middlesex Victoria Rifle Volunteers.
Lamprey, Jones, M.B., Surgeon 67th Regt. 1*l*.

MILITARY BREECH-LOADING SMALL ARMS.

By Captain V. D. Majendie, R.A., Assistant Superintendent, Royal Laboratory, Woolwich.

The present time would seem peculiarly favourable for the consideration of the subject which is set down for discussion this evening. Not only has public attention been recently forcibly drawn in this direction, but we stand at this moment in a position which gives to the subject, as far as this country, at least, is concerned, a peculiar completeness.

Considered broadly, the subject falls naturally into three main subdivisions—the past, the present, and the future of breech-loading arms. The ancient history of breech-loaders has already been dealt with in this room; most of you know with how much ability and care. The future of breech-loaders is a question of much speculative interest, and one which branches off into several lines of curious enquiry. It is one, too, of high importance, in its connection especially with the probable influence of the new arms upon military movements and organization, and upon the relative probable value of infantry, cavalry, and artillery. But this evening my remarks will be limited as closely as possible to the middle phase—the *present* of breech-loading; nor shall I go further back or further forward than may be necessary to ensure connection and completeness.

The advantages which breech-loading presents over muzzle-loading for military small arms are, as we shall presently see, so great that its employment amounts to nothing short of an instinct. So much more natural is it to introduce the charge of a gun at the breech than at the

muzzle, that a very large proportion of the early hand guns were so loaded. The system, no doubt, was abandoned mainly on account of the greater perfection in mechanical workmanship required in breech-loading arms—a perfection scarcely attainable in those early days, but which the gradual development of the force of gunpowder rendered more and more indispensable. The development of the system was hampered also by other considerations which will be more apparent as we proceed, but which sufficed to neutralize in a great measure the advantages which we should otherwise have derived from its adoption.

Looking at the subject from the ground which we now occupy, it may justly create surprise that, with one important exception, every military power in Europe has been content hitherto to rely upon a muzzle-loading musket for the general equipment of its troops. In truth, however, the subject had never, until lately, been fairly faced and considered. It had been viewed in a half-light, experimented upon from time to time with languid interest, cramped within contracted limits, misunderstood, suspected. When the instinct of which I have spoken had been blunted by a different habit, men had not set themselves diligently to reckon up the abstract advantages of a breech-loading military arm. It was a thing of the past—or it might be even a thing of the future, but never a tangible reality of the present. One reason of this indifference is to be found in the fact that each nation considered itself with its muzzle-loading rifles at least as well off as its neighbours, or so far in advance of them even, as might be measured by the superior accuracy and perfection of its arms. These muzzle-loaders, too, had won for us some scores of great battles, and had served as efficient tools wherewith the greatest commanders in the world had worked their way to fame. Finally, their supersession would entail an expense and an amount of anxiety, and possibly of difficulties, which there seemed no sufficient reason for incurring.

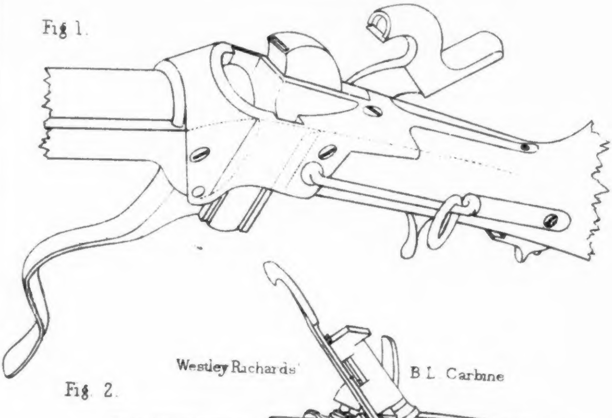
It must be remembered that the change involved in the supersession of a particular class of military arm is a very serious one to make: serious, because of the interests and issues involved; especially serious in our case, because of the scattered nature of our forces, and the variety of climates and conditions to which our equipment must be suited. What country, except England, has to provide ammunition, for example, which neither the intense cold of a Canadian winter, or the heat of an Indian summer, or the searching damp of the Indian rains can injure or affect; which under these opposite influences will still give the same sort of results with which the annual performances at Wimbledon have familiarized us, and which the fastidious criticism of a free competition and a free press have taught us to require? Inertia has to be overcome by all new ideas and inventions, as by all material things, before they can pass from that state of rest in which sometimes to our loss, no doubt, but not seldom to our benefit, so many of them eternally remain. I do not know that the military profession is specially open to reproach because it does not admit an opposite law. When occasion demands the required force is generally forthcoming. It may be the force of public opinion; it may be the

force of science; it may be the force of progress and events, or the force of intrinsic merit; and it is undoubtedly better in the main for the country and ourselves, that we should yield only to these, and not be sensitive to the mere touch of every ingenious invention, or to that weak, restless craving for change and ideal perfection which is not to be confounded with real progress.

Those who believe that breech-loading rifles formed no part of the equipment of the British army until the present or the past year—that they sprang directly out of the performances of the needle-gun in the late Bohemian campaign, will learn with surprise that not only had breech-loading been determined upon for our infantry nearly three years ago, and the actual pattern of arm that is now in the hands of a large proportion of our troops, decided upon a full month before Königgratz was fought, but that two regiments of English cavalry were provided with a breech-loading carbine ten years ago. If we examine one of these Sharp's carbines, as they are called (Plate x, fig. 1), we notice at once an objectionable feature, viz., that the ignition is effected by means of a percussion cap. Then, again, the arm is provided with no *effective* arrangement for checking the escape of gas. So great is this escape, that a handkerchief laid over the breech at the moment of firing, will be burnt through and through; whereas with a close-fitting arm or cartridge, the handkerchief should not be even soiled by the discharge: here for example is a piece of calico which has been laid upon the breech of a Snider rifle for 40 rounds. This defect is aggravated by the quantity of powder, which in damp weather adheres outside the breech after loading. The back end of the cartridge is cut off thus, and the powder sprinkled about. From these two causes, the firer's face is generally flecked and burnt in firing; while the escape of gas tends also to foul and clog the breech action, which exposes large friction surfaces, and to render the arm more difficult to open as each round is fired. For these reasons, the retention of the percussion cap; the liability to an escape of gas; the flash occasioned also by the spilt powder; and the difficulties which often arise in loading, this system is extremely imperfect. And yet this system did undoubtedly reflect to some extent the state of feeling which prevailed in this country on the subject of military breech-loading arms a few years ago. For at that time this question was regarded exclusively as a cavalry question, and quick shooting was scarcely thought desirable for a cavalry soldier. It was desirable no doubt that he should be relieved of the inconveniences which attend muzzle-loading on horseback—inconveniences so great as to be almost ludicrous, when we think of a man under fire trying, on the back of his horse half-maddened by noise, excitement, and terror, to pour gunpowder down a narrow tube, to ram down a bullet, and to adjust a percussion cap on the nipple of his rifle. But the cavalry soldier has hitherto been required to be essentially a swordsman, not a trigger-puller; and so much emphasis was at one time laid upon this consideration as to engender, what I cannot but regard as a very dangerous undercurrent of opinion, that the efficiency of the hussar or the dragoon would be best assured, by providing him with a sharp sword and a not too effective fire-arm.

Sharp's Breech loading Carbine

Fig 1.



Westley Richards' B.L. Carbine

Fig. 2.

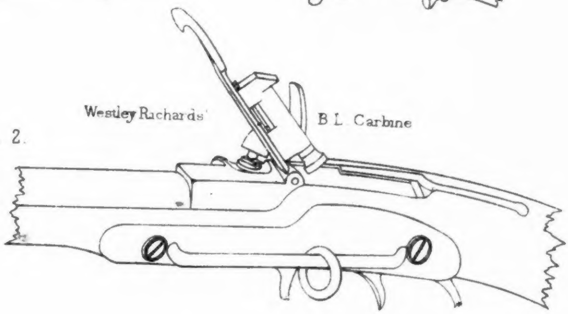
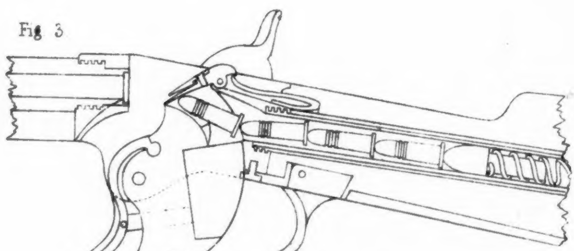


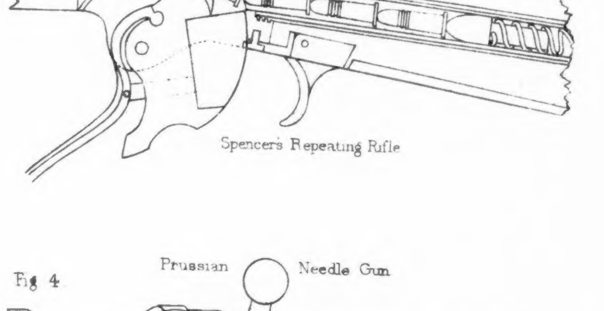
Fig. 3.



Spencer's Repeating Rifle

Prussian Needle Gun

Fig. 4.



J Jobbins

Between 1857 and 1861 three other breech-loaders were introduced for experimental cavalry use. One of these, the Green's carbine, even in that easy age, never obtained a footing as a recognized service arm. But the Terry carbine, which I have here, was introduced to some extent, and is not yet entirely obsolete; and the Westley Richards' carbine (Plate x, fig. 2) has found justly more favour. In 1861 this arm was definitively adopted. It is a great improvement on the others which I have named. It is an accurate arm; it fires six or seven rounds a minute with comparative ease and certainty; it spills no powder; it does not burn the firer's face or facings. Until the recent competition, Westley Richards' was accepted as the best type of military breech-loading arm known in this country, and, with its many objections, I would still pronounce it one of the most effective capping breech-loaders which has been produced.

But we have now to consider the subject in a broader light. It had been treated hitherto from one narrow point of view—as almost exclusively a cavalry question. We had considered only arms of which the percussion cap formed a material element, and a capping arm may be pronounced, with our present knowledge and for our present purpose, only half a breech-loader. Thus far, then, we may justly say that breech-loading had been cramped within contracted limits, and had been misunderstood. But a couple of years or so ago, the whole aspect of the case altered. As by a revelation we learnt then, what breech-loading really meant, and of what development the system was capable. It was a great epoch for breech-loading, its Hegira, it might be called, when it was discovered that the objections to cartridges containing within themselves the means of ignition, had in reality no force. If we are to trace the stagnation which had hung over the question, to a definite cause, we may confidently place our finger upon these objections.

When men said, and the ablest and most experienced military men did say, and our military authorities laid it down as a fundamental axiom, that cartridges containing their own ignition were not admissible for military use, what they meant was: 1st, That such cartridges were more liable than others to accidental explosion; and 2nd, That in the event of the explosion of one cartridge, the contents of the barrel were liable to be all exploded *en masse*, and so to communicate from barrel to barrel. If we grant these premises, the conclusion is just. Ammunition which is liable to explode in bulk, and thus not only to commit injury and dangerous havoc for the moment, but by its explosion to deprive the troops dependent upon it of their supplies, is clearly not admissible for military use. But when, mainly, I believe, by reason of the explosion of gunpowder at Erith two years and a half ago, men's minds were directed towards explosions generally, an experiment was made to determine the liability of small-arm cartridges —not breech-loading cartridges in this case, but the ordinary muzzle-loading Enfield rifle cartridge, ball and blank—to explode in bulk, and it was established not only that the explosion of a single cartridge in a barrel was not communicated to the rest, but that the explosion of a number of cartridges, or even of $\frac{1}{4}$ lb. of loose powder, although it might burst open and destroy the barrel, would not occasion

a general explosion. Although the bearing of this fact upon the question of breech-loading was not immediately perceived, it furnished the opening through which presently a flood of strong new light rushed in upon the subject.

This effect proceeds from the same cause as that which operates in rendering the well-known Gale gunpowder inexplosive. Mr. Gale enclosed each grain of his powder in an incombustible envelope of finely-powdered glass or bone-dust. You know, doubtless, the result. Here is some powder rendered non-explosive by the introduction of four parts of powdered glass. I stir the mixture with a red-hot rod, and as you see without exploding it. If I separate the powder from the diluent, we at once, as you see, restore its explosive character. Now in a barrel of cartridges we have not each grain enveloped, but a number of grains, so many as compose one charge, in a non-combustible case. I have tried over and over again to explode a barrel of the service breech-loading cartridges without success. I have several times fired one cartridge in a barrel without igniting the remainder. I have fired ten cartridges at once, and no more have exploded. I have gone further, and placed the barrel inside this iron cylinder, tightly screwed down, and have exploded $\frac{1}{4}$ lb. of powder in the midst of the 700 cartridges which it contained; and although the screws were broken and the lid of the cylinder was blown off with violence, and some of the cartridges were strangely distorted, none of the cartridges were ignited. Experiments not less exhaustive have established also the non-liability of these cartridges to separate accidental ignition by concussion or otherwise. In this way we dispose of the objections which have been entertained against cartridges containing their own ignition, and establish their admissibility for military service.

Although between 1859 and 1864 no less than 26 different plans of breech-loading were proposed; although for sporting purposes breech-loading guns had found their way into very general use; and although in America they were being largely applied experimentally for military use, this shadow always hung over the question—that cartridges containing their own ignition were not generally considered admissible for military use. And as breech-loaders not adapted for such ammunition presented comparatively few advantages except for cavalry, for whom as I have explained, quick firing was not specially desired, and as the development of the system was thus stunted, the question was in fact argued in a circle, and the subject remained up to this period practically at a stand-still.

But when this fatal restriction and limitation were removed—we began at once to make real progress in the matter. It was no longer a question merely of an arm which presented certain minor facilities of manipulation—of an arm somewhat handier to load on horseback—but it became obviously a question of an arm, which would multiply the fire of an army three or fourfold—which, properly considered, would place as it were three or four rifles in each soldier's hands.

In the course of the Dano-German war the value of the famous needle-gun (Plate x, fig. 4), or rather of the system of which the needle-gun was an indifferent exponent, became in the eyes of observant men fully

established. So obvious was the teaching of this campaign, that the then Secretary of State for War, Lord de Grey, forthwith appointed a committee, with General Russell as president, to "report upon the advisability of arming the infantry, either in whole or in part, with breech-loaders." After four meetings this committee reported, abstractedly and without reference to any particular system, that it would be desirable to arm the whole of the infantry with breech-loading rifles; and on the day on which this report was drawn up, the 11th July, 1864, the death-warrant of muzzle-loading rifles for the use of the British soldiers may be regarded as having been signed.

The next question to be considered, was, how to give effect to the recommendation of the committee. This question admitted of consideration in two ways: either by itself with a view to ascertain the speediest and cheapest mode of placing a breech-loader in the hands of the troops, or in combination with other questions connected with rifles, such as the best size of the bore and the best mode of igniting the cartridge, and with a view to determine what would be, in all respects, the most perfect arm for the use of the infantry. The first mode of dealing with the question would have the advantage of celerity, the second of completeness. But why not have combined the two, by arming the British army immediately with the needle-gun? Here was a complete system—a good system, perhaps, some will say, ready-made to our hand; a system too which had had the advantage of having been tried on actual service, and which was certainly inexpensive. I have here a needle-gun which will I think answer this question for itself. Take the gun as it stands, and tell me—those of you at least, and I doubt not there are many, who are accustomed to handle the beautiful weapons which England puts into the hands of her soldiers, her volunteers, and her sportsmen—tell me if this clumsy rifle is one which you would have cared to see issued to our troops.

But why not, you will say, improve the workmanship of the piece—make it lighter, balance it better, alter its bore if need be, or its mode of rifling, improve its mechanism, and when you have suited it to the more advanced requirements of this country, adopt it? Need I point out to you that such a measure would have disposed of the first argument for the adoption of the arm. We are no longer in this case adopting a system ready made to our hands. We are in fact creating one. We are adopting a breech mechanism, merely one element of a system, nothing more. And but little knowledge of the subject was needed to instruct us as to the extremely defective nature or principle of this breech mechanism. If I handle the needle-gun you will see that its action is comparatively slow. It is clumsy and imperfect, in other ways. The needle with which the cartridge is ignited is very liable to become bent or injured. I shall be reminded that these needles are easily replaced, that each Prussian soldier carries two or three. But this injury and replacing of a needle, temporarily disables the arm, and constitutes, it must be admitted, an objection. Then again, the gas-check is not permanently reliable. The whole escape is thrown upon the arm, at the junction of the breech, none upon the cartridge; and even if we set out with a tight fit, the fit will become less and less

close, and in time, if the arm be not carefully looked to and repaired, an inconvenient escape of gas will occur,—an escape which sometimes induces the Prussian soldier to deliver his fire by preference from the hip. Of the ammunition it will be sufficient to say that it is rude, and for technical reasons ill adapted for the requirements of military service. The egg-shaped bullet is embedded in a small papier-maché wad, which serves the double purpose of giving rotation to the bullet, which never touches the grooves, and of containing the fulminate, which the needle, penetrating the powder and the thin paper envelope which encloses the whole, has to pierce. In short the needle-gun, however superior even to a *good* muzzle-loader, was not such an arm as we should have been justified in adopting, except on an emergency more pressing than that which now presented himself.

Of the other systems of which we had any experience, and which you have seen, there was none which seemed calculated to satisfy our requirements, and their adoption was accordingly not entertained.

Under these circumstances, having no complete system to begin upon, we determined to take so much of a known and reliable system as we approved, and apply it. By this course we should get, as it were, a lift upon our road, and start somewhat in advance of the point from which we must set out, if we elected to consider the question *ab ovo*. Therefore, and without prejudicing the ultimate and more leisurely investigation which the question as a whole demanded, we resolved to take so much of the existing arm—the Enfield rifle—as seemed to us good, and to revolutionize that part of it which we deemed bad. We determined, in short, to convert our Enfield rifles into breech-loaders. In speaking of the Enfield rifle as a good arm, I am anxious not to be misunderstood. It is an arm doubtless with many defects, if we may judge it by the high standard of more modern requirements. A steel barrel, for example, would probably be preferable to a wrought-iron barrel; the Enfield twist is undoubtedly too slow for extreme accuracy; and the calibre may be considered as unnecessarily large. The refinements and progress of gunmaking have left the Enfield rifle to a certain extent behindhand, just as the refinements and progress of breech-loaders have left the needle-gun behind; and yet in the main it is an excellent weapon for military use, and I know no power whose soldiers possess a muzzle-loading rifle which can compare with it. We do not, recollect, require a match-rifle for military purposes. Except, on a few rare occasions, such an arm would have no special value. It was Lord Palmerston, I think, who pointed out that what the soldier is required to do is, not to hit a particular button upon his enemy's coat but generally to drive home an effective fire into an opposing body of infantry or cavalry.

Extreme accuracy is of course a good thing to have in a military rifle, just as well as to have a watch which will beat time like a chronometer; but for the ordinary circumstances of warfare, a rifle of average precision, say of the precision of the Enfield rifle, will answer all our purposes, just as a chronometer movement is not absolutely essential in every-day life. If we can get the extreme accuracy in either case, without sacrificing other qualities or without great outlay, well and good—but men exposed to a searching fire, hurried, wearied,

confused, surrounded by smoke and strange noises, wounds and terror, will fail generally in giving full effect to the precision of which even a moderately accurate arm is capable.

Instructed then by the sober teaching of actual warfare, we were justified, I think, in assuming, that if to this Enfield rifle, without subtracting from its accuracy and other qualities, we could apply an effective breech-loading arrangement, we should be at once relieved of all pressing anxiety, and in a position to select leisurely a new and superior weapon for future manufacture. Moreover, there were more than half-a-million of these muskets available for conversion; and the step which was proposed had the secondary, but by no means inconsiderable advantage of economy. It had other subordinate advantages, such as the experience in breech-loading equipment to be derived in the course of the enquiry, and which might stand us in good stead when the more complete and difficult question should come before us, and finally, the avoidance of the danger of hurrying without due consideration, into the adoption of a possibly unsatisfactory arm.

In August, 1864, an advertisement was issued, inviting gunmakers and others to submit propositions for the conversion of the Enfield rifle. From the fifty systems which were sent in in reply to this advertisement, those which were obviously unsuitable were eliminated, and eight systems were selected for trial, of which five only, for reasons which it is unnecessary for me to detail, ultimately came to the post. Of these, four were capping arms, one only a non-capping arm. The selection for trial of these capping arms, indicates that we were not then as advanced or decided upon this point as we are now, and had not yet absolutely learned to regard non-capping as a *sine quâ non*. So Westley Richards', Mont Storm's, Wilson's, and Green's converted Enfield rifles entered the lists against the solitary representative of the non-capping system—the Snider rifle.

Westley Richards' converted Enfield, was substantially the same in respect to its breech mechanism as the cavalry carbine which I have shown you, with the addition of a small hook at the end of the plunger for withdrawing the wad after firing.

In Wilson's arm, the breech of the original rifle is removed, and the barrel prolonged for some inches in the form of an open slot. The cartridge is inserted here, and pushed forward into the barrel, and is followed by a sliding plunger, which is fixed after loading by a stout bolt which passes through stock and plunger. There is an india-rubber ring to diminish the escape of gas.

The Green rifle resembled the Wilson, except in the manner of securing the plunger, which is furnished with a small knob, and is turned round after loading, a quarter circle, and so locked.

The Montgomery Storm, or Mont-Storm arm is one of that class of breech-loaders known as "chamber-loaders," and of which I shall have to speak again presently. The chamber of the Storm rifle can be turned completely over the breech, and loaded; or it may be swivelled at right angles to the barrel for loading with loose powder. When the chamber is returned, it is secured by a bolt worked by the lock, and an escape is prevented by an expanding ring or thimble on the pan of the chamber.

In the first arms made on the Snider system, about two inches of the upper side of the breech end of the barrel were cut away, leaving a wide open slot or trench for the admission of the cartridge. When the cartridge had been pushed forward into a taper chamber formed by enlarging what was now the hind part of the barrel, the slot was closed by a lump of steel, hinged on the right side of the barrel and forming a false breech. It was afterwards found more convenient to remove the back part of the barrel bodily, and to replace it with a "shoe," in which the whole of the breech arrangement was comprised; other modifications followed, until we got at last, the more perfect arm which I hold in my hand (Plate xi). The breech-block, you will see, hinges upon this side pin and works backwards and forwards. It is kept in its place by a small spring stud; and the fit of the block tends also to hold it.

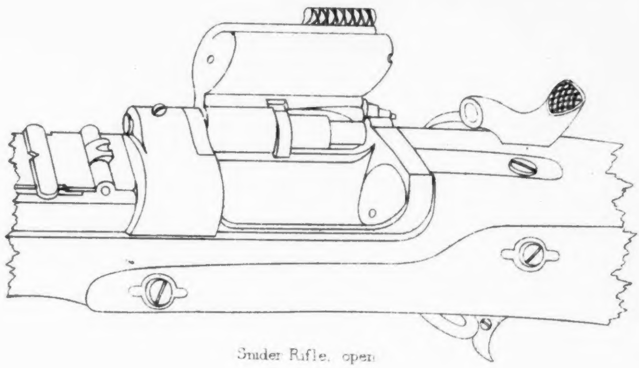
In order practically to meet a possible objection that this arrangement is liable to wear and get out of order, I have brought to-night for your inspection, two Snider rifles. One of these rifles has fired, under my superintendence, 15,082 rounds; the other has fired 17,082 rounds; or if you would go further still, I have here a "shoe" which has never even been hardened, and yet which has fired 30,000 rounds and still remains, as you will see, if you care to inspect it presently, perfectly serviceable. Nor have these rifles and this "shoe" been tinkered up for exhibition. On no occasion have they become injured or even temporarily unserviceable—and although springs and pistons do sometimes break, I can only say that these springs and pistons throughout all this practice, have not once required renewal or repair.

The ignition is effected by means of a small piston or striker, which passes through the breech-block, and which, when in repose, is flush with the face of the block. A blow of the hammer causes it to dart forward about a tenth of an inch into the cap, which is fixed, as I shall presently more particularly explain, in the base of the cartridge. The piston is returned by a spiral spring. To withdraw the empty cartridge case, a claw or extractor forms part of the breech-block. When I withdraw the block, the empty cartridge is necessarily drawn with it, and by canting the rifle sideways, the case is thrown out. The extractor is returned by another spiral spring.

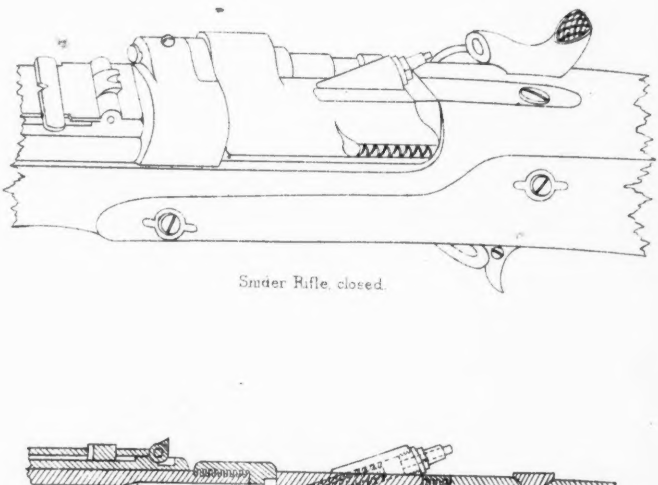
It has been stated lately that the Snider system was not really invented by Mr. Snider at all, but by M. François Eugène Schneider; or to go further back, by Mr. John Poad Drake, a Cornishman. But if we are really to trace the system to its source, we must go back to a time which places the invention quite beyond the reach of living men. I have not thus far ventured on the archæology of breech-loading; but if you will permit me, I will make one dive into antiquity for the purpose of bringing under your notice two breech-loading firearms of the reign of Henry VIII., on the Snider system!* By the kindness of Admiral Caffin, these interesting arms have been lent from the museum of the Tower, for the purpose of exhibition this evening.

In the course of the competition, the Snider gun proved about 50 per cent. quicker than its rivals; it was stronger too; it was simple, and

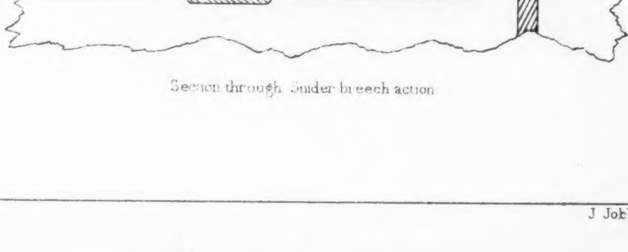
* For an illustration of such an arm, see Journal of the Institution, vol. ix, Plate xv, fig. 3.—Ed.



Snider Rifle, open.



Snider Rifle, closed.



Section through Snider breech action.

J Jobbins

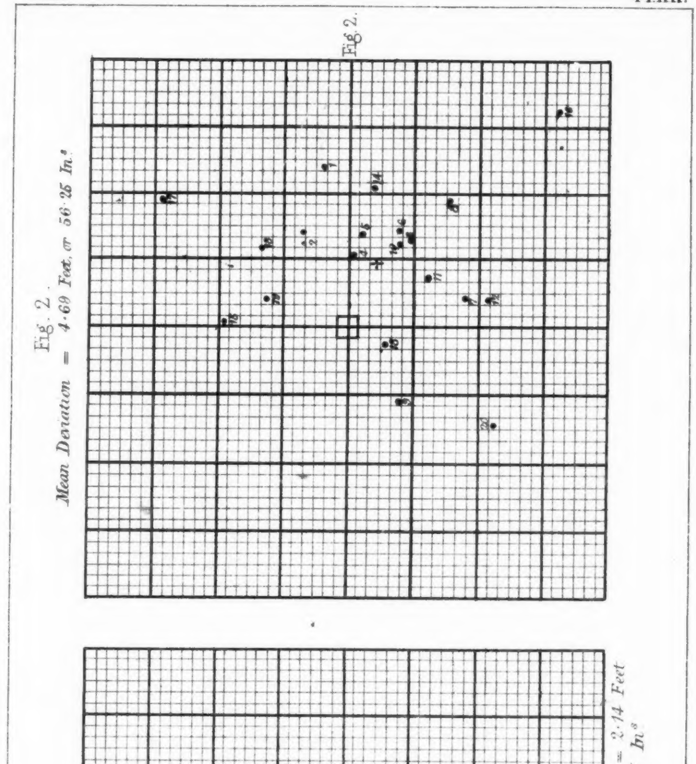


Fig. 2.
Mean Deviation = 4·69 Feet, or 56·25 In.s
Fig. 2.
Fig. 1.
Mean Deviation = 1·56 Feet
or 18·35 In.s
Mean Deviation = 2·14 Feet
or 25·65 In.s
Fig. 3.
Mean Deviation = 1·07 Feet
or 12·85 In.s
Fig. 4.
Fig 1.
Fig 3
Fig 4

J. Jobbins

apparently durable ; and, lastly—I should perhaps have placed this advantage first, not last—it was adapted for a cartridge containing its own ignition.

Among the capping arms, the Mont-Storm rifle was ranked first, and was recommended for experimental application to a certain number of rifles. The system failed subsequently at proof, and the recommendation was cancelled. It failed partly on account of the unsuitability of the skin cartridge which formed part of the system; and this reminds me of another objection to which all capping arms are open. Such arms need a cartridge so thin, that the fire from the cap shall pierce it, and at the same time the cartridge must be so entirely consumed or carried out by the discharge, as to leave no residue to endanger or interfere with loading. These requirements make it difficult to satisfy another not less important point in a military cartridge, viz., that it shall be strong enough to stand the knocking about to which it will inevitably be exposed in transport and on service. Moreover, a thin cartridge is evidently less well adapted than a stout one to resist the effects of an accidental adjacent explosion. But the Mont-Storm rifle failed also in the arm itself. Under proof charges, the hinge and small bolt by which the chamber is locked, were broken.

The Snider rifle, while satisfying many requirements, failed in one important respect. It was so inaccurate as to be quite unsuitable for adoption as it stood.

These diagrams (Plate xii) will show you the extent of this inaccuracy. This one (Fig. 1) represents the mean shooting of an unconverted Enfield at 500 yards, the large squares upon the target being 3 foot each. Here (Fig. 2) we have some of the first shooting of the Snider rifle in the competition; here (Fig. 3) some better, although still bad, when a slight change had been made in the ammunition. At this point then, as the fault obviously could not lie in the barrel, the question of providing more suitable ammunition for the arm, was referred to Colonel Boxer.

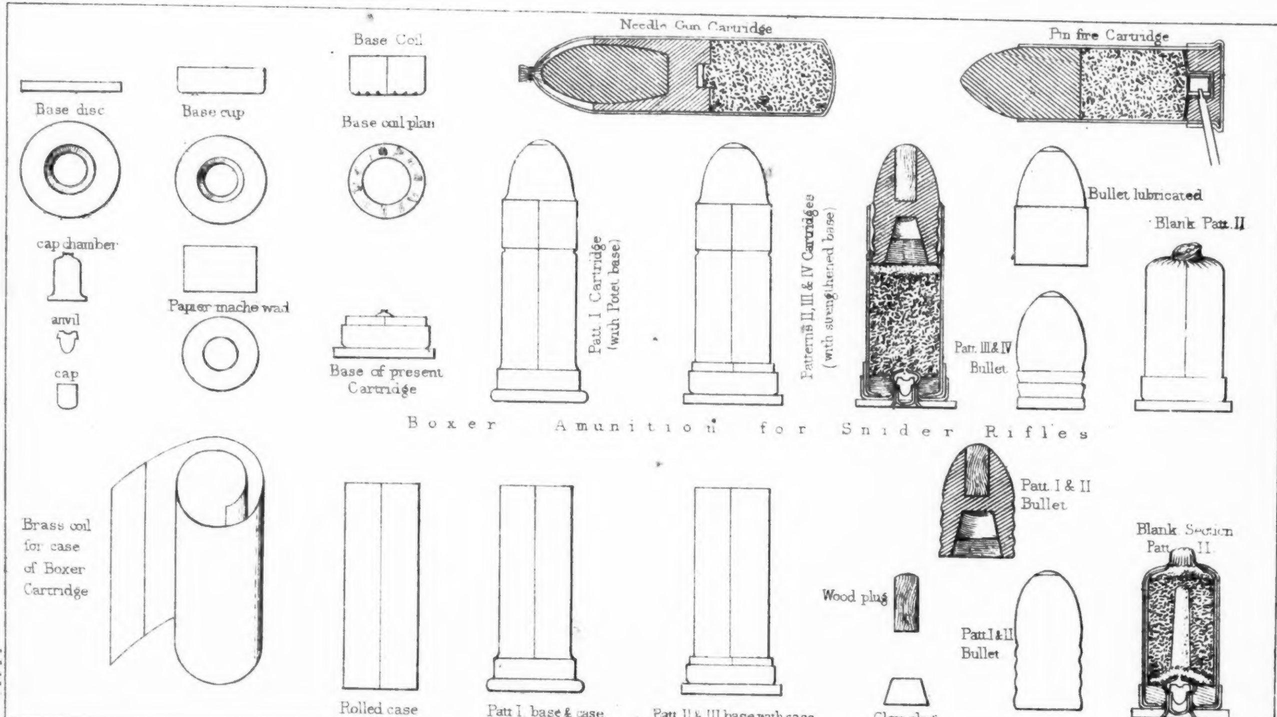
Discarding the papier-maché case submitted by Mr. Snider, the objections to which, especially in damp weather, are well known, Colonel Boxer made the case of his cartridge of very thin sheet brass, ·003-inch thick. The cartridge has a little over two turns of this brass.

Five important advantages result from the employment of this case: —1st, Being uncoiled slightly by the explosion, instead of depending upon the mere stretch of the material, the case can be used in a chamber considerably larger than itself, with little danger of breakage and consequent escape of gas. 2nd, It is not liable to swell with damp, and so to interfere either with loading or withdrawal. 3rd, The difference between the size of the case and of the chamber is so considerable as to admit of loading even when the case has become considerably enlarged or disfigured by rough usage. Nor under these circumstances again, is there any danger of leakage or escape. 4th, Such a cartridge may be made, like the present service cartridges, practically waterproof. 5th, The reaction, or tendency of the case to recoil, which arises after the pressure of gas is removed, tends to render a case of this sort easier of extraction than any other. All of these advantages

hold good against a papier-maché case; many of them hold good also against a case of simple copper or other metal. Cartridges of this last sort, have been extensively used in America. But the weight of bullet and charges used in that country is very much less than we are accustomed to require, and it is very doubtful how far such cases can be made, due regard being had to the three important considerations of weight, economy and easy extraction, strong enough to stand the strain involved in the use of our heavier projectiles and high charges. I am not speaking now, remember, of an universal cartridge, but of a cartridge adapted for the class of arms of which the Snider rifle is an extreme type, arms in which the cartridge becomes at the moment of firing just as strictly the breech or chamber of the gun, as this iron chamber in the gun of Henry VIII.'s reign. The only difference consists in the fact, that while in Henry VIII.'s reign, men preserved and reloaded their chamber, we throw it away after each discharge and take a fresh one; though, as sportsmen know, for economy's sake, we too sometimes use a chamber a second time, and refill it as often as it will serve us. My objections to copper cartridges are urged, however, specially in connection with this system or principle of breech-loading; but as regards papier-maché cartridges, we may, I think, speak more confidently, and pronounce against them a general verdict of disapproval for military use, on the grounds of want of durability and of the power to resist the effects of moisture.

The only change which has been made in the Boxer cartridge since its introduction, has been the substitution of a strengthened base, the details of which are shown on the diagram (Plate xiii) before you, for the original Potet base, which when manufactured in large quantities proved not altogether reliable. The ignition is that usually adopted in central fire cartridges, a small brass chamber in the centre of the base, containing an anvil, over which is placed the percussion cap. The expression "central fire" is applied to this arrangement in contradistinction to "pin fire," or to "rim fire," or to that method of ignition adopted in the Prussian system, which for want of a better name we may call "needle fire." By pin fire is meant the old Lefaucheux system which is generally applied in sporting cartridges, in which the anvil becomes, as it were, the striker and projects in the form of a pin. In rim fire cartridges, the fulminate is situated in the rim or flange of the cartridge, and although this construction presents undoubtedly certain advantages, it tends, by throwing some additional strain upon a weak part of the case, to produce fracture; and the Americans, who have hitherto been the main supporters of rim fire, are now, I believe, for this and other reasons, very generally substituting central fire, the details of which, however, would seem to be still open to considerable simplification.

The last point to be noticed in the ammunition for the Snider rifle is the bullet. On this the accuracy of fire of the arm depends. The bullet is not made, as in the majority of breech-loading arms, slightly larger than the bore, but depends for its action upon the system of expansion which Colonel Minié was, I believe, the first practically to adopt, viz., a hollow in the base, together with a plug, by which the original Minié iron cap has been superseded. This plug, which is



Boxer Amunition for Snider Rifles

J Jobbins

now made of baked clay, plays the double part of expanding and supporting agent. The expansion of the bullet would be effected, it is true, to a great extent by the simple action of the powder-gas upon the sides of the hollow. But a plug makes that expansion more instantaneous and more uniform; and, above all, it supports the sides of the bullet after expansion. Thus, with a plug, the passage of the gas is prevented and fouling diminished, in the first place; and, in the second, even when fouling has been established, its effect upon the accuracy of a plugged bullet, whose sides do not collapse when they come into contact with the obstructing deposit, will be much less than upon an expanded bullet which has no plug.

Another important feature of the bullet, is the wooden plug in the head. By this plug we obtain three advantages: greater length, and so a broader bearing and lubricating surface; secondly, the centre of gravity is more favourably adjusted with reference to the requirements of the projectile and the slow twist of the Enfield rifle; thirdly, the weight is disposed, as in the fly-wheel, away from the axis of rotation. This construction is well known in its common application to shell bullets, for which, indeed, it is a matter rather of necessity than of choice. Norton, Jacob, Dyer, Metford, and others, have all made use of it, and Mr. Metford and Mr. Whitworth have both employed it successfully for purposes of accuracy. It is important, however, to understand, that this feature by itself, does not constitute the essence of the bullet. It plays its part, together with the other features of the construction, and contributes its quota of accuracy; but the ultimate success of the projectile is due rather to the accurate adjustment and balance of the whole combination, than to any particular feature of the bullet taken singly. Those only who have followed the enquiry from its infancy can be aware of the nice points upon which the accuracy of shooting of this arm depends. Here, for example, are two bullets. Nine persons out of ten in this room would probably be unable to detect in them any note-worthy difference; and yet, one of these bullets shoots well and the other badly. And I would have you remark that the key to whatever difficulties have temporarily arisen in respect to the precision of the arm, is furnished by this explanation. It was because the accuracy depended upon the exceeding niceness of the combination—I am speaking now with reference not to the bullet only, but to the bullet in its relation to the arm and to the powder—that a loss of accuracy ensued, when that combination was departed from in the slightest degree, necessitating for the long Enfield the adoption of another bullet with a larger margin of efficiency.

Round the bullet are disposed grooves or cannelures, which serve to carry the bees' wax lubricant, by which means a layer of wax is always interposed between the lead of the bullet and the sides of the bore; and fouling is so completely got rid of, that one of the best targets ever made with this arm was made after 1,000 rounds had been fired from it without cleaning. This point is one of extreme importance, for the accuracy of a military arm is to be measured by its average accuracy during a long sustained fire, and not by its performances when perfectly clean. The measure of efficiency of a military arm in this, as in other

respects, is to be obtained by taking the arm, not at its best, but at its worst. In other respects, the ammunition satisfied the tests which were imposed. It stood an extraordinary amount of rough usage. It was waterproof to an extent which enabled it to be kept for a whole week in wet sawdust without injury; it was easy of extraction; not liable to escape or explosion; and its expense is very little greater than that of a paper cartridge, even if we take its first cost merely, and immeasurably less, if we spread the cost over the periods during which the two ammunitions would respectively remain serviceable—if indeed, the paper cartridge could ever be considered serviceable. The extreme rapidity of fire in the arm is fifteen shots per minute. Of this ammunition about 11,000,000 of rounds were issued up to the close of the financial year, which ended last Saturday week, and of the arms 136,431.

To deal with the false and exaggerated reports which have been circulated respecting "the failure" of the Snider system, is a task which I cannot undertake, except in a general way. I can only say that in essence, and in substance, these reports are false. The system is to be considered from three points of view: as a breech-loader, as an arm of precision, and as a system of conversion. As a breech-loader—and every breech-loading system consists not of the gun merely, but of the gun and ammunition—the system has proved a most complete success. The percentage of casualties in this respect, as we gather from the reports of the troops in Canada, at Hythe, at Aldershot, has been absolutely inappreciable. For rapidity of fire, safety, simplicity, easy loading and extraction, and non-liability to get out of order, the system has proved all that was anticipated.

With respect to the rifle as an arm of precision, the difficulties which occurred for a short time, and which were experienced, it should be noticed, only with the long Enfields, have been easily and successfully overcome. It may tend also perhaps to restore confidence, if I state that lumping together the casualties of all sorts, grazes, mis-fires, split cartridges, &c., which have occurred out of a total of 94,840 rounds fired, up to the present time in the course of the ordinary daily proof, they amount only to 216, or under 1 in every 400 rounds. And the present accuracy of the arm is represented by this diagram (Plate xii, fig. 4). With respect generally to the system as a system of conversion, we certainly do not exaggerate its advantages when we say that it has given us, with the precision of the original Enfield rifle, from three to four times the rapidity of fire, at a cost of something below £1 per arm.

The progress made meanwhile by our neighbours, as far as I have been able to gather reliable information, is shown by this table.*

We are now able to observe the completeness which the question has for the moment attained. Having provided an effective arm for the *immediate* use of the British army, we are in a position to discuss calmly, although not too leisurely, the question of its *future* equipment, and you will judge, that materials for its consideration will not be wanting, when I state that the Sub-committee on breech-loaders have now before them, no less than 93 different systems from which to select the future arm of the British soldier.

* See Table on page 2 7.—Ed.

I have collected a few characteristic specimens of the different modifications of which breech-loading is susceptible, and which will serve to illustrate the classification which I propose to attempt. It is of course impossible to exhibit these arms in detail; but, together with the very rich collection belonging to this Institution, they will be open to your inspection after the lecture.

Breech-loaders may be divided into two great classes:—(1) breech-loaders, in the ordinary acceptation of the term; and (2) repeaters.

The first class may be further subdivided into (*a*) chamber-loaders, and (*b*) breech-loaders proper. A chamber-loader is in fact a sort of muzzle-loader cut short, or so arranged that the arm can be conveniently loaded by hand, without the assistance of a ramrod. The charge is deposited in a short chamber, instead of being rammed all the way down a long barrel; and the chamber is then replaced in the position for firing in the prolongation of the barrel. Of breech-loaders on this plan there are many modifications. In the Mont-Storm arm, the hinge is in front of the chamber; in many rifles (the Sweedish Hägstrom for example) it is behind, and that would probably be the most primitive form of chamber-loading. Colt's and Deane and Adam's revolvers are examples of chamber-loading arms having several chambers. In these arms, the chambers are not hinged, but revolve, and are brought in succession into prolongation of the barrel. I have several other examples of chamber-loading arms here, to which I would invite your attention after this discourse. There is a Spanish arm (Garcia's), Bergstrom's, and another Norwegian system; Leetch's, Mackenzie, and Wentworth's, Lechmere's, Standermayer's, and many others which I cannot even find time to mention.

The more conspicuous defect of this class of arm would seem to be their liability to injury by the explosion, which generally acts directly upon the breech mechanism. On the other hand, they are generally perfectly free from escape of gas, except sometimes at the junction of the chamber and barrel. In Sweden and Norway, chamber-loaders find more favour than in this country.

Breech-loaders proper include more numerous types of arms. The Snider rifle is an arm of this class; so is the Westley Richards, the Terry, the Sharp, the Green, the needle-gun, all of which you have seen. I have here several others, some good, some bad, some celebrated in their way, and some which exhibit the imperfections of which the system is capable. The Amsler-Milbank system (adopted by the Swiss Government for conversion), Joslyn's, Bayliss's, Restell's, Beard's, Burton's, Prince's, the Starr carbine (now in use in Canada), Fosbery's, Laidley's (adopted in Russia), are all examples of breech-loaders proper. In these arms, as a rule, very much more work is thrown upon the cartridge. Where, as in the needle-gun and the Sharp carbine, the escape is sustained by the gun, and not by the cartridge, the system will generally be open to objection on the score of excessive escape of gas. With secure ammunition, this defect may be entirely got rid of; and to me it seems sounder in principle and more reliable in practice to throw the burden of the escape upon the cartridge, which

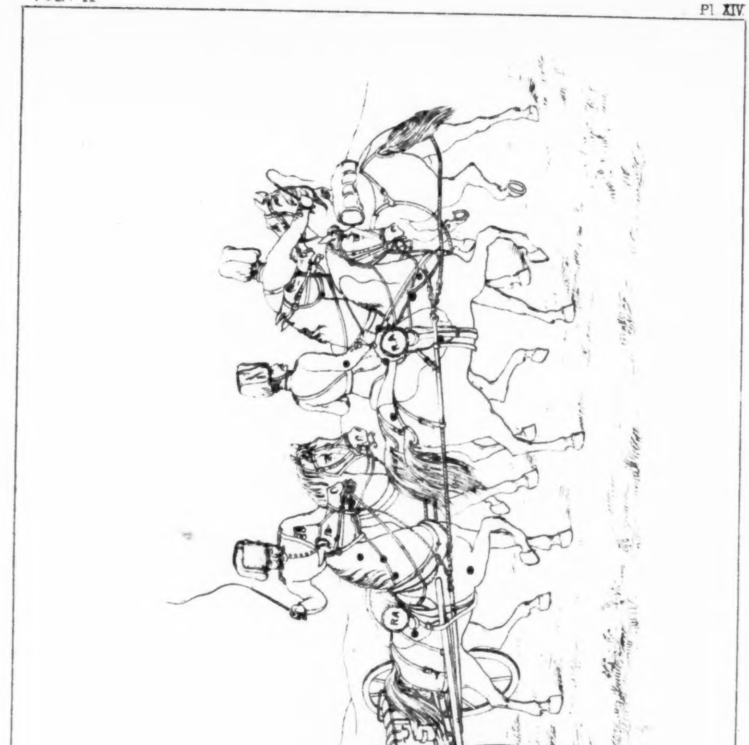
only has to sustain it once, than upon the arm, which must sustain it many thousands of times.

Repeaters may be subdivided into (*a*) revolvers, and (*b*) magazine arms. Of Revolvers the best examples are furnished by revolving pistols, of which, by the addition of a long stock and barrel, rifles have sometimes been made. I have here also a revolving carbine, by a Colonel Porter. The system is open to many objections, among which, except for pistols, the weight of the several chambers is conspicuous.

Of Magazine arms there are two important varieties: The simple repeater, such as Henry's, in which the cartridges are constantly drawn from a magazine under the barrel, which must be replenished from time to time as it becomes exhausted; and secondly, an arm which, like the Spencer and Lamson rifles, can be used as repeaters or as simple breech-loaders at will. In these arms I can, if I wish, shut off the magazine, and load them as ordinary breech-loaders. This plan presents several important advantages over the ordinary repeater, which entails distinct intervals of inefficiency while the magazine is being replenished, and which directly tempts the soldier to indulge in an excessive rapidity of fire while his magazine supply lasts. But in the improved repeaters, these objections are obviated. I have here, for example, the Lamson rifle, with its magazine full. I need not, however, call upon the magazine, but I may load as if the arm were an ordinary breech-loader. Then when pressed, I can, so to speak, turn on the tap of the magazine, and pour forth such a fire as no simple breech-loader can deliver. When my magazine is exhausted, I am not, as with an ordinary repeater, temporarily disabled, for I can fall back upon the simple breech-loading action, until an opportunity presents itself for replenishing the magazine against another emergency. So with the Spencer rifle, in which, as you will see from this diagram (Plate x, fig. 3), the magazine is situated in the stock.

The direction in which repeaters generally err, is in complexity of construction; but if this defect can be overcome, a magazine-rifle would present immense advantages over the simple breech-loader, not merely for those services, such as the navy, the cavalry and artillery, in which an intensely rapid fire is generally required for a few decisive moments, but for the universal equipment of troops. It is in arms of this class, that breech-loading tends towards its highest development; and to this principle of action I believe we must look for the ultimate solution of the breech-loading question.

In selecting from the numerous systems of breech-loaders one for military use, certain general considerations must always be observed. Rapidity of fire is of course essential; and so should be perfect safety and freedom from escape of gas. The arm should not be easily liable to injury from rough usage, fouling, damp, or the action of discharge. Facility of loading and extraction are merely necessary elements in rapidity of fire. Close fitting, or broad sliding surfaces are to be avoided. A moderate degree of accuracy combined with lowness of trajectory, and penetration should be insisted upon. The ammunition should be safe, durable, if possible waterproof, perfectly reliable and



J Jobbins

serviceable in different climates—and *after* all this (not before) as cheap as you like. And these conditions require to be satisfied not merely on the parade ground or in the showroom, but under the severe tests to which in actual warfare, the system will be exposed. The problem viewed in this light will appear—as I would have it appear—less easy of solution than is popularly supposed.

The advantages which breech-loading presents in a military rifle are—

First, *Rapidity of Fire.* We give each soldier, so to speak, and as I have before expressed it, three or four rifles, with the inconveniences only of one. At close quarters no troops, however brave, devoted, or disciplined, could stand with muzzle-loaders against a corresponding force armed with breech-loaders. It amounts to being opposed to a force whose numbers are practically multiplied by the figure which expresses the ratio of rapidity of fire of the breech-loaders to the muzzle-loaders. We must not press this argument too far. It will hold generally when the fighting is quick and close and decisive, and when the conditions of the contest on both sides are the same. But breech-loaders will not do everything; and we must avoid the error of supposing that they did everything in the Danish or Bohemian campaigns. The needle-gun was but the embodiment of that spirit of "*geist*" and progress which animated the Prussian army and its leaders, which dictated the execution of their rapid movements, and which was the soul and essence of their superior organization. Especially must we avoid the too close application of mere abstract reasoning when the element of artillery fire comes into play. Eleven years ago an experiment was made at Hythe with life-size dummy figures of men and horses, which went to prove conclusively that artillery would be beaten off the field by infantry armed with rifles. This drawing (Plate xiii) shows what was then proved, that a detachment coming into action would be annihilated in three minutes by thirty file of riflemen. But two important considerations were overlooked: that in actual warfare, infantry would scarcely deliver so effective a fire as then served to plant at 800 yards' range, thirty-four shots in a single gun detachment,—just as two men standing opposite one another at twenty paces to fight a duel, often fail to hit one another, while at a very much greater distance they can each easily break a bottle's neck in a pistol gallery. And, secondly, the necessity for artillery coming within this range at all, was not established; so that the reasoning which was based upon these premises fell in practice to the ground; and notwithstanding improved musketry instruction and improvements in the arms, the use and importance of artillery have in nowise diminished since the introduction of the rifle for general service: so those also would be mistaken who might argue from this diagram that if so much could be done with muzzle-loading rifles, firing as they did on this occasion one round per minute only, breech-loaders firing seven or eight rounds per minute would produce a corresponding effect, and that the predominance of artillery fire in an action must henceforth cease to exist. But within reasonable limits, the effects of breech-loaders, as opposed to muzzle-loaders, can hardly be over-

estimated, It must be remembered too, that there is always attendant upon the employment of a more effective arm, a moral as well as a physical effect. As Marshal Marmont said, a battle is decided after all, not by the number of men killed, but by the number frightened.

By a converse application of this argument, we reach the second advantage of breech-loading, *increased confidence*, a point upon which I need not dwell, but of which all military men will recognize the importance.

Thirdly, we obtain greater *facility in loading*. On horseback, breech-loading, even of an imperfect kind, is obviously vastly superior mechanically to muzzle-loading. For the infantry soldier, if we reflect a moment, the advantages will appear quite as great. Whatever the soldier's position, whether lying behind some sheltering mound, cramped in a rifle-pit, working in close squares, with his bayonet presented to resist cavalry, or running forward as a skirmisher, he can load a breech-loader as he could never load a muzzle-loader—without exposing himself, without changing his position, without inconvenience or loss of time or effect.

We have, in the fourth place, *improved shooting*. The arm is always loaded in a position which favours the subsequent delivery of a low and effective fire. The eye is never removed from the object; and no part of the powder can be spilt, no part can be lodged in the grooves of the rifle; while the increased confidence of which I have spoken tends to steady men's arms and improve their aim. It has been objected that the rapid firing of the breech-loader will tend to tire and unsteady man's arms, but surely this objection, if it has any force, may be met by the consideration, that the operation of loading a breech-loader is very much less fatiguing.

Fifthly, *the possibility of overloading is avoided*. This, in the hurry and excitement of action is no uncommon accident. A man loads and as he thinks fires. His cap misses fire, or he even neglects to cap at all. But he does not at once recognize it. He rams down another charge; perhaps another, &c. &c. After one of the American battles, several arms were picked up loaded with two charges, others with three; some with four, and a few even with eight!

Among the minor advantages of breech-loaders, I may name the completeness and compactness of the ammunition; the facilities for cleaning and inspecting the arms; the ease with which the drill may be acquired; the diminished danger in loading; and the impossibility of the arm being rendered inefficient by the loss of a ramrod.

It has been said that breech-loaders will entail an excessive expenditure of ammunition. But in the first place, we have no grounds for supposing that the ammunition expended, will be *wasted;* and a similar argument would have held against the supersession of the old flint-lock by the percussion cap. If the fire be delivered at such ranges that the shots tell, it merely amounts to this, that the work is done with the new system so much quicker and more effectually than with the old; and experience teaches us that the requisite supply can be kept up without difficulty, even in a long hot general action, and that this objection has been much overrated. It is stated indeed that

the greater coolness and confidence of the men tend rather to a less expenditure; and the comparatively small number of rounds fired by any individual Prussian soldier in the late campaign, if figures are to be relied upon, would seem to favour this view. The supply of ammunition, however, whatever its expenditure, is only a question of organization; its *efficient* expenditure is a matter of instruction.

BREECH-LOADERS

Already adopted, or in Contemplation by the different Military Powers.

	System of Conversion.	New Arms.	Remarks.
England ..	Snider	Not Determined	
France.. ..	Nil	Chassepot	
Austria ..	Wänzel	2,000 Remington's Ordered for Trial	The New Arm not finally decided.
Prussia ..	Nil	Needle Gun	
Spain	?	?	
Portugal ..	?	Westley Richards	
Sweden ..	Nil	Hägstorm	It is said that Sweden proposes to adopt the Remington for future manufacture.
Norway ..	?	?	
Belgium ..	Snider	Not Determined	Several Arms under trial.
Switzerland ..	Amsler—Milbank	Winchester	
Turkey ..	Snider	Lamson	The new arm uncertain, but Lamson has been named as probable.
Russia.. ..	Terry	Laidley	
United States..	?	Berdan Spencer	
Canada ..	?	Peabody Starr Spencer	
Italy	?	Westley Richards	Large numbers ordered.

The CHAIRMAN: Gentlemen, this is a subject that very often entails a difference of opinion; but I think there can be no difference of opinion as to the talent

Capt. Majendie has displayed in the paper which he has read to us this evening. At the same time there may be different remarks and opinions which gentlemen may wish to express, and which I am quite sure Capt. Majendie, and every one else present, will be glad to hear.

Colonel EVELYN: As no gentleman appears desirous of coming forward, I rise to make a few remarks—not to express a difference of opinion from Capt. Majendie, for I think I agree with almost every word of his lecture. But I think there was one point of view which is generally lost sight of in the consideration of the great advantages to be obtained from the breech-loading gun. I think we shall never attain the full advantage of quick loading till we get rid, in some manner, of the smoke. The great advantage to be obtained at present from the breech-loading system does not depend on the rapidity of fire, but on the ease of loading, and upon the fact that the man is, as you may say, never unloaded. Now I have stood in front of a line of men firing blank cartridge, and after the second round not a man in the line has been able to see me. That proved that even with muzzle-loaders the fire in line was too quick. But supposing the spot I occupied had been occupied by an opposing body of troops armed with guns firing with some smokeless material, their fire would be directed by the flashes of the opposing guns, and they would be able to fire as accurately as if there had been no smoke at all. In point of fact, it is the smoke caused by the firing of one's own gun that obstructs one's view, not the smoke of the enemy. Therefore the attention, I think, of all men interested in this subject should now be directed not so much to the improvement of breech-loaders, as to the attainment of a smokeless compound. We shall never get the full advantages of quick loading till we get that. One of the dangers we ought now to avoid is that of loading our men with an enormous quantity of ammunition. It does not follow, as I think the lecturer observed, that there should be any increased number of rounds fired in consequence of troops being armed with a superior weapon. If so, in what does the superiority consist? If it requires a greater number of rounds to produce the same effect, it would be rather a proof that the weapon is not superior. I doubt whether there would be any greater number of shots fired in action than there used to be; but I know the system that has hitherto obtained with regard to the second and third supply of ammunition has been very inefficient in our own and other services; but I believe that has been immensely improved. I maintain that fifty rounds per man is as much as a soldier ought to be loaded with, until we very much reduce the weight of our cartridges. The American troops during the last war were only armed with fifty rounds, and I do not see why there should be more. Remember, the soldier has to march with his ammunition; he has to go on guard; he has perhaps to carry that ammunition for a month, day and night, before he fires it; and to add two or three pounds to the weight in that particular, would impede very much his efficiency. I think it would be a great pity, if we were to have all sorts of extra belts and accoutrements for the purpose of supplying our unfortunate infantry soldiers with extra ammunition, when the first reserve should always be at hand. The quantity of ammunition the soldier carries, should be influenced entirely by the quantity he can bear without inconvenience, and not by the number of rounds he may have to fire in a general action, for the first reserve should always be at hand.

General ABBOTT: I should like to ask Capt. Majendie a question with regard to a statement which I did not quite understand. It was with regard to the precision in firing of the bullets used for the present arm. I have understood that two descriptions of bullets are required; one for the short rifle and one for the long. Under these circumstances, would there not be great inconvenience experienced in serving out the ammunition for the short and for the long rifle? It appears to me there would be a good deal of inconvenience occasioned, and I should like to hear what explanation Capt. Majendie can give.

Brigadier-General LEFROY, R.A., F.R.S.: I can, I think, Mr. Chairman, answer that question. It is the fact that the bullet used in the long Enfield is a 480 grain bullet. Now, that does not shoot quite so well with the short Enfield as the 528 grain bullet originally introduced, but the difference is so small that it would be quite unnecessary to perpetuate two ammunitions for the sake of it. The difference does not amount to more than two or three inches at 500 yards distance, which, I think, most practical men will think quite near enough.

Captain STRANGE, R.A.: I wish to make a few remarks with reference to what was said by the first speaker as to reducing the weight of the ammunition. It might be, that the size of the bullet may be considerably reduced, because we know that a man in hospital is worse than a dead man, and a wounded man also has to be carried to the rear. Not only that, but there are two fellows always very ready to carry him to the rear, and sometimes another might go with his cap. That has been remarked before. I do not mean to say, of course, that British soldiers are at all addicted to this kind of thing in general. The remark would be only applicable to those opposed to our fire. The fact remains, that wounded men are a greater incumbrance to an army than dead men, who are very easily got rid of. We must, however, consider that the bullets, if made smaller, might not be effective as regards horses. I just mention those two points because they have not been alluded to.

Capt. GOODLIFFE, City of London Rifle Volunteer Brigade: I wish, Sir, to ask one or two questions. First, I think the Lecturer stated that the cost of the present ammunition, as settled for the Snider rifle is equal, if not much in excess, of that at present supplied for the Enfield rifle. I do not know that I understood him distinctly on that head, but I thought that was one of the points he insisted upon. It appears to me to be a very important one indeed. A second question also is, that the Government seem rapidly to have taken up the conversion of the Enfield rifle, without any reference whatever to improving its construction, beyond making it a breech-loader. It has long been held by those acquainted with the Enfield rifle, that it fails in one or two most important particulars, which might easily be remedied without any detriment to the arm itself, and would produce much greater accuracy in shooting. I allude of course to the inefficiency of the twist; that is to say, the twist is made too slow to enable a bullet to be carried at long range, with anything like accuracy. Another point is with reference to placing the back sight of the rifle so far to the rear. At the present moment it requires considerable practice before it can be efficiently used. The back sight is probably the most clumsy and senseless contrivance that could possibly be suggested, because it is impossible to take that accurate aim which might easily be obtained by a blackbar slide, which, indeed, is usually adopted in match rifles. There appears to be no reason why these improvements should not be carried out with reference to the Snider rifle. But we must, I think, first of all make up our minds whether or not mere accuracy of aim, or mere rapidity of fire, is all that is to be desired. If you want accuracy of shooting, you will never attain it with the practice at present given to the British soldier. Not being a military man myself, I am not able to state the reasons why; but if the accuracy of the aim is to be of any importance, as I apprehend it will be more and more so, it will be necessary to give a certain contingent out of every regiment, a much higher degree of qualification and instruction than they have at present, because the small amount of practice which the soldier has, is quite insufficient to render him a practical and skilled marksman. It appears to me you must first of all obtain something like accuracy in the weapon, and then give that instruction which will enable the soldier to use it efficiently. I see no reason why this should not be done with the Snider rifle, but the difficulty at present is in the cost of the ammunition, which seems a very formidable one, and unless it can be shown that the ammunition can be constructed at a much lower cost than we have already heard stated, it will be a very serious objection. I have not heard any answer to those very remarkable letters which appeared in the public press from General Hay, with reference to the bad shooting of the Snider rifle. It is perfectly clear that in a military weapon, low trajectory is an important feature, and according to General Hay's statement, the trajectory was not only much too high, but the bullets went, as he very forcibly expressed it, "nowhere." Now, if the Government is going to convert rifles at an enormous expense, the bullets of which are to go "nowhere," and the trajectory which, as far as it can be ascertained, is "anywhere," it seems to me to be a very serious mistake. No doubt the Lecturer will be able to give us some explanation, but, coming from so high an authority as General Hay, it is a question demanding the most serious consideration, and if General Hay's statement be true, it appears to me to be almost fatal to the success of the Snider rifle, as a weapon of accuracy.

Mr. G. Daw: I should like to ask whether Capt. Majendie considers these specimens (the specimens on the table) of a perfect cartridge, and whether they are the cheapest that have been furnished? I ask it because I see a great variety of breech-loaders have been shown, but we have only one specimen of cartridge. I must say I think it would have been a little more to the enlightenment of the company if we had had a variety of cartridges also. As the matter stands at present, one would suppose that that was the only cartridge that had been introduced. I make these remarks for this reason, that as I was the pioneer of that system into this country, I certainly should like to have seen the different principles fairly discussed if time had permitted, because Colonel Boxer has thought proper to christen that cartridge with his own name, and I think it is important to us as ratepayers to find out whether we have really in the service, the best and cheapest cartridge.

Mr. Clark: I wish to offer a few remarks upon one part of this subject which I believe is admitted to be of very great importance. I refer to the cartridges which play as important a part in breech-loaders as the gun itself. There were two distinct descriptions of cartridges; those with metallic cases, waterproof and capable of being submitted to rough usuage, and the ordinary paper cartridge. I think in this discussion the properties of the two cartridges, their respective cases, ought to be taken into consideration. The difference of expense between the two cartridges is very considerable, and one that bears heavily upon the finances of the country. It has this importance, that a difference of expense in the gun itself, once incurred, is done with, but the difference in the expense of the cartridges is everlasting. It is imposing a great outlay upon the expenditure of the country. Now I am able to go to figures with some degree of accuracy in the case of the two descriptions of cartridges. It is pretty well known by volunteers—that is a test of what the real cost of the cartridge is—that the paper cartridge for the muzle-loader is delivered at 5*s*. per hundred, 6*d*. for the packet of ten. And it has come to my knowledge that the volunteers have heard with great dismay that when the day for arming them with breech-loaders shall come, and of course that day is not far distant, they will have to pay, instead of 5*s*. per hundred, 10*s*. per hundred for their cartridges. Now if we go into that question in a broad point of view as applied to the expenditure of the country, the importance is really of very great magnitude, and we may say in general terms, that the difference of cost between the two cartridges is £2 per 1000. Now as a million of cartridges is composed of a thousand times that number, there is no less a difference than £2000. In the last session it was stated in the House that forty millions of cartridges were ordered for breech-loaders, and if we apply the figure I have just mentioned £2000 per million, there is £80,000 difference of expenditure for the article of cartridges alone. Another point bearing upon the same question which was very aptly referred to by a gallant Officer on the other side of the theatre, is the weight of the cartridge to be carried by the soldier. Now the difference between the ordinary paper cartridge and the "Boxer" cartridge is very considerable. I see by the printed notice to inventors and gun-makers calling upon them to enter into competition, one of the conditions is that the weight of the metallic cartridge of the breech-loader is not to exceed six pounds four ounces for sixty rounds. I believe that weight was the weight of the "Boxer" cartridge before it was found to be too weak to stand the force of the explosion, the weight has since then been considerably increased, and I believe I am not wrong in saying the sixty rounds now weigh, instead of six pounds four ounces, about seven pounds. Perhaps that may be a little above the mark, but at all events the weight is considerably increased. The difference in weight will amount to about a pound and a-half between the paper cartridge and the metallic cartridge. That, you will admit, is very considerable. I wish to draw your attention to those two points—the cost of the cartridge and the weight which certainly bears in a very important point of view upon the question we are discussing.

Capt. Selwyn, R. N.: I beg to move the adjournment of the discussion, convinced as I am that there are many persons here present who desire to offer still further remarks, as indeed I do myself. I am not at all disposed, however ably the paper has been put before us, to allow many of the statements to pass uncontradicted. I beg therefore to move the adjournment of the discussion.

The Chairman: I do not know that we can call upon Captain Majendie or any

one else to attend again because we may wish to have an adjourned meeting. Captain Majendie is kind enough to come here to-night, but I do not know that we can ask him to attend again.

Captain SELWYN: It has been the rule hitherto in this Institution that whenever a paper has occupied so much time as not to leave a reasonable time for discussion, which in this instance only amounted to about a quarter of an hour, that course has been followed. Therefore I am not, I think, out of order.

The CHAIRMAN: Not the least out of order. Is there any seconder?

The motion for the adjournment was seconded by Captain Moncrieff.

The CHAIRMAN: I think the point to settle, first of all, is whether we can call upon Captain Majendie to come here again to answer questions. I myself do not think it is quite right, when a gentleman volunteers to come to read a paper, that we should prolong the discussion on it. The lecturer is quite willing to answer anything that can take place in the course of the hour, but I do not know that we can ask him to come here again for the purpose of continuing or answering a discussion. That is my impression.

Captain BURGESS: I rise, Sir, to say that our discussions have often been continued till nearly eleven o'clock at night.

The CHAIRMAN: It depends very much upon the time of gentlemen. We must not go beyond a certain reasonable hour. At the same time I have no doubt Captain Majendie will wait. I hope you will not be too long.

Captain SELWYN: I am sorry that I am so ill as to be almost unable to speak to night. However, I think the question is of the highest importance, and that if the public are left under the impressions which Captain Majendie's lecture would give them in very many points they would be uninformed, and in still more points they would be misinformed. First as to the accuracy obtainable by the Snider rifle. Now this has been made one of the features of excellence supposed to belong to this particular system, brought forward in a very peculiar way. To those who know what the principles of artillery, are I need only affirm, I am sure they will support me in saying it, that the shooting is due to the barrel and projectile, that if you had your chamber containing your gas a mile off from your barrel and had the same force of gas, the same projectile, and the same barrel, you would get exactly the same shooting as if the chamber was part of the barrel as long as there was no escape of the gas. Therefore, the mode of enclosing the gas as long as the gas does not escape, does not matter one iota, unless you adopt any particular form which is best calculated to throw the gases in the proper direction. That is the only thing that can make a difference. The history of the Snider rifle is a very peculiar one. A Committee was appointed, and it was laid down as a principle to start with by that Committee, that self ignition cartridges were utterly to be despised and condemned. Under those conditions, that competition took place, and it was only from America that there came certain systems of self-ignition cartridges, which were, when first examined, thoroughly laughed at, and were not accepted at all. I hold here in my hand a report of that Committee which distinctly awards the palm in every respect, save that of self ignition, to the Mont Storm rifle. I presume that those Mont-Storm rifles were tried by the usual proofs, and I do not think the Committee have usually been in the habit of trying any rifle that has not been subjected to proper proof. Many thousands of those arms have been manufactured less than the average number bursting in proof; I am not an owner or the inventor of the Mont-Storm system, but simply anxious for fair justice to every one, of the number of those guns proved amounting to many thousands, rather less than the average per centage burst. Since these competitive trials, the Committee take occasion to compliment Mr. Mont-Storm on the perfection to which he has brought his rifle, and to say in all respects, except that of self-ignition, it is the one they recommend. It appears rather late to say now, that the rifles failed in trial, which they did not do till long after all these trials had been gone through. It appears very strange instead of saying, "Can you give us one thing more, and then we shall be quite satisfied,—that is, self-ignition?" instead of doing that, they turned round to the very worst rifle, one of the most complicated, one that had been condemned and refused by every other government in Europe before it was brought here, and by adding to it Mr. Mont-Storm's shoe,

which was shown to them by the manufacturer of the Mont-Storm rifles, by adding to it various other little plaisters here and springs there, they at last brought out a rifle which according to the recent reports, as just now remarked, has a trajectory "anywhere," and shoots "nowhere." It does not always do so; how is that? The lecturer tells us in his hands it has accomplished a wonderful accuracy, but I should like to know what was the diameter of the bullet employed? Was it the ordinary Enfield ammunition to which the rifles were restricted that fired during this competition, or was it an ammunition specially constructed for it with a bullet of a larger diameter, with very little windage, which will make any rifle shoot better up to a certain point, and which is a thing which ought never to be lost sight of, seeing that windage was introduced to patch up a difficulty which we found in loading our rifles? The bullet is made up, it appears, of wood in front, wood in rear, and lead distributed about the body, and is supposed to shoot better than a lead bullet. I should like to know, if that be the case, why we do not fire some metal that has low specific gravity? What is the reason we have always chosen lead, the material having the greatest specific gravity? It seems to me that if I take a cork bullet and fire it out of a rifle I shall get a very high trajectory, and I shall reach a very short distance, and the bullet will be nowhere. That will be the only result of diminishing the specific gravity of any projectile, while I left its diameter the same or rather increased. That is one of the laws of artillery that I do not think anybody will contest. Then it appears that the cartridge was not quite satisfactory even after the change was made from the original cartridge proposed by Mr. Snider, to the cartridge manufactured by Colonel Boxer,—that was not satisfactory, for the base expanded a little too much,—that is to say, it exploded occasionally. And, more than that, another thing was, that as the mechanical conditions are unfulfilled in the extractor, that is to say, that you do precisely what no boy would think of doing if you were to give him a box of sugar plums and asked him to take one out,—put his finger without his thumb down at the side; you put your extractor on one side of the case, a thing which is known to be a fallacy to every sportsman who has used a central fire cartridge for the past three or four years or more; you pull at one side of your cylinder, you push it over on your chamber, and you get an unsatisfactory extraction. To meet that you patch on a little bit of brass, but that happens to weigh something, and you find that your cartridges exceed the weight you allowed to the competitors; and so you proceed ingeniously to lighten the ball. That is one way out of the difficulty, of course if people like to take it; but I do not think it is in accordance with any correct principles, and I am quite sure that if we pass from the cartridge to the arm, we shall find we have gone far from taking the right principles. We have taken a portion of the barrel and practically thrown it away, and we have now placed the ball, instead of being where it ought to be, two inches farther forward, two inches up the barrel, so that the ball lies under the sight. Still farther, we have coned out the barrel towards the rear, which is about as bad a proceeding as we could adopt, in order to facilitate the withdrawal of the cartridge. Thus they have patched up little difficulties one after the other. The question of price has been so very ably dealt with by Mr. Clarke, that I shall not refer to it at all. I merely recognise the correctness of his figures. £2 per thousand is the difference in price. What the difference in cost is at Enfield it is impossible to say, because I do not know whether interest on capital or depreciation of plant are taken into consideration; but to the volunteer it amounts to this, that when he has fired away a thousand cartridges he has fired away the cost of two guns. That is about the history of it. If you can get a solid punched metal cartridge which will obviate the difficulties of the present one, there is no earthly reason for retaining all those folds of metal and bits of paper, pasteboard, and one thing or the other which mount up to fourteen different parts in the construction of that cartridge. If you ask the Americans here to-night, they will say that they go in for simplicity, and so long as they can punch their metal they prefer to do that to having a folded sheet, putting paper over it, a cap, and a bell cup, and an anvil, and a bit of pulp to keep that in them—putting three brass bases at the end, and calling it a perfect cartridge. What we ought to go in for, is *first principles*, and not this or that invention. I understood Captain Majendie to say that with regard to paper cartridges, he considered them as totally

out of the field by the fact of their liability to injury by explosion or by damp. Now I think perhaps he will limit that remark as regards explosion, if the thickness of the paper be increased. I should say then that probably the paper cartridge may be as little liable to explosion as the metal cartridge, because I know the paper will bend and allow itself to be deformed as much as the metal. But there is another difficulty when we come to the thicker cartridge, and that is that you increase necessarily the size of the chamber, and those objections must be overcome before we can allow it to take its place in competition with perfectly punched metal cartridges, which I am convinced can be made by the manufacturing skill of this country in any shape that is desired. Now the expediency of meeting the transition state of things which Captain Majendie has very ably put, and for providing for the case in which our soldiers may either be called upon to fire loose powder and ball or special cartridges, is one which no doubt deserves great attention, and ought not to be neglected. It is possible to fulfil both those conditions, to give the soldiers an arm which will be equally well able to fire with a cap and loose powder and ball or with its own special cartridge, giving the very highest rate of firing and the lowest price of cartridge. The question as to the Prussian needle guns and the other guns which have been brought here this evening, has, I think been very ably disposed of, showing that those foreign guns have been surpassed in this country. We need not waste time in considering any of those arms which firing a non-metallic cartridge that is blown away, have an imperfect gas closure, and throw the strain of the explosion on the gun and not on the cartridge, for the whole of those difficulties are at once met by the fact of the substitution of the metal cartridge in whatever form we may have it. I think I may lay it down as an axiom that we must have a metal cartridge of some description, and I hope to see it introduced as well for sporting purposes as for the work of the army. To the Volunteer, the question of refilling, is an important one. If you have cartridges which can be refilled five or six times, it reduces the price of his cartridges practically below the price of the paper cartridges which he had before. That will be a very important point to fulfil, and that also I believe can be done with punched metallic cartridges. I now draw attention to the question of increased accuracy, and I beg to say I would take the arm which made that target and put on to it any one of the breech mechanisms which do not admit of any wasteful expenditure of force by the escape of gas, and I would get precisely the same shooting from it under the same conditions of barrel and projectile. Therefore it should not be considered that the chamber of the gun has anything to do with the shooting of the weapon. We have been told that this was a mere question of a stock in trade. Now I mean to say, a proper breech-loading arm, such as has been offered to the Government before now by various parties, all meritorious more or less, would be capable of having 20 or 30 barrels screwed into it as fast as the rifling was destroyed or worn out; and therefore it should never have been regarded as a question of the stock already existing. We might have had the very best arm as a conversion as well as we shall have it two years hence. There is only one other point I wish to refer to, and that is the liability of the gun to destruction at sea. Now you are going to supply us, as the result of this competition, and you are supplying us as the result of this conversion, with a rifle to be used as well at sea as on shore. I lay it down without fear, as an uncontradictable principle, that no spiral spring will ever stand at sea. You may have a rifle for six months exposed to a sea atmosphere and see no change; all of a sudden you take it out and you see your springs are snapped. This was spoken of not long since in this Institution by Captain Colomb, who found that he could not rely upon steel spiral springs, and that even brass spiral springs could not be relied upon. After the lecture on that evening was over, Mr. Nock, the celebrated gunmaker's son came to me and said: "Sir, I can only say my father would never allow me to put a spiral spring in a gun." Those are matters of experience not to be lightly despised, which I think should occupy the attention of Government before they allow any unnecessary complications to be introduced in the rifle which has to win our future battles, as far as small arms can do it by sea and land. The other parts of the Snider rifle are also exceedingly liable to displacement. I do not say they are so in such a gentleman's hand as Captain Majendie's, who, like myself, has probably had fire-arms in his hands from

boyhood, but in the hands of the common soldier. First of all, if there is any gas escape—and the best manufacturers of cartridges in the world cannot turn out a thousand cartridges, one of which may not have gas escape—if there is any gas escape at all, the result is, the pin is blown back and the breech-block is blown open, which in fact occurred the other day with the Snider rifle in Belgium at Government trials. They tried damaged cartridges and the result was, that every Snider breech-block they tried, was blown open by the explosion of the the gas. I do not at all agree with Captain Majendie's idea that the hammer holds the breech down. It does not do so in the slightest degree. If the striker is blown back at all, the result is, that the hammer is raised, the whole of the breech block arrangement slips from under it and the breech blows open. I know several accidents have occurred from that cause. Then there are three spiral springs that we have to deal with, and I shall perhaps be told that they are not important. Certainly they are an objection; every additional piece of mechanism is an objection. Then again we have an extractor, and although it may be very cleverly managed with a sham cartridge, yet when you come to load and fire that gun, you find in nine cases out of ten you have to make three motions; first, to draw the extractor back by hand, secondly, to release it, and then if you are lucky it will go forward, but if it is a little rusty, it won't. Next you have your cartridge back about an inch, and then you find you have to put your finger to take it out. It is very nice to exhibit experiments, and to tell us that used in practised hands at Woolwich this has not occurred, but I know it has occurred in many hands, and it will occur again, and if you can extract without any such arrangement you will have done very good service. I have touched as far as I could on the cartridge and also on the arm. I will not detain you longer except to observe on the fact that for some reason or other, the ramrods are retained in these guns up to the very latest date of competition. I hope it may be made the subject of some thought, whether for the theoretical advantage of spitting a fowl, or cleaning a barrel which you ought not to want to clean except by a common bit of string and a bullet, or for hooking cartridges out when they stick, which we have heard it has been greatly used for at Woolwich, it is worth while to retain a ramrod. I have heard one man say that out of sixteen cartridges he had to push twelve out of the barrel with the aid of the ramrod, these were only experimental ones of course, but I hope, however, it will be considered whether it is worth while for these purposes to carry a ramrod in a breech-loading gun. If we are to carry more cartridges with a breech-loading gun, then we can do so only by diminishing the unnecessary weights, and we could diminish the weight of the gun so much as not to make the increased weight of the cartridges sensibly felt.

Major-General Boileau, F.R.S.: I wish to ask one question with reference to the calibre, whether on general principles any approximation to the future calibre of our breech loading rifles has been adopted. I mention this because the calibre of our rifle is the largest of all breech-loading rifles in use at present. The Americans adopted a rifle with half an inch calibre, and the Chassepot has a calibre of ·433. There is a Chasseport rifle in the room which has been brought over by Mr. Christy. I believe it is the only one in England, and it is a rifle of Mr. Chassepot's own manufacture. There are also two rifles of recent American invention by Mr. Hammond, which Mr. Daw has brought, and perhaps Captain Majendie will not object to their being exhibited, either now or by and by. I think we have had as regards "the present of breech-loading rifles," a most lucid and interesting paper from Captain Majendie, a paper which although I have for nearly seven years been dabbling in rifles, has given me food which I do not expect to digest before the next lecture comes off in this Institution. I think also as regards future rifles that the Committee have quite enough before them in studying the 93 patterns lately sent in, and I have no doubt they will give us the best they can. But the question of calibre has not been mentioned this evening, and it appears to me to be one by which the future of breech-loading rifles will be greatly affected. I shall be glad to hear therefore whether any approximation to the future calibre of breech-loading rifles has been agreed upon.

[Mr. Christy here exhibited a Chassepot rifle, and said it was a specimen of the

actual arm adopted by the French Government, of which 10,000 had already been issued to the French troops. After describing its mechanism very briefly, and having illustrated his description by taking the breech of the rifle to pieces, he said that the French Government had decided to do away with all screws that could be at all troublesome to a man in a camp after a day's campaign. He had heard that the spiral spring was objected to by some gentlemen, but the spring was so thoroughly protected in the Chassepot rifle, that it was not at all objected to in France.*]

Captain C. O. Browne, R.A.: There is one question Captain Selwyn brought forward, which I think should be answered, as it had reference to Artillery. He said that if it was imagined that by having wood in the centre of a bullet it would shoot better, the same argument would hold good for making the bullet entirely of wood or cork. Now, if he had been an Artilleryman, I should have appealed to him to ask whether he did not know it was a fact that a rifle shell carries much more truly than a shot.

Captain Selwyn: To what range?

Captain Browne: To any range.

Captain Selwyn: Certainly not; you ought to be artilleryman enough to know that.

Captain Browne: At all events, we will say that at most ranges the shell carries more truly. The inside has shellac, air, and powder, an artilleryman would hardly argue from that that a projectile of the density of air, powder, and shellac would shoot better still. Had he said he was a mathematician I should have told him the "radius of gyration" was lengthened in a hollow bullet. I think he did say he was a mechanic and an engineer. Mr. Whitworth is a mechanic and he seems to have found out that a hollow bullet shoots best. It has been said to-night that the spin of the Enfield rifle is too slow already; in order to keep up the spin the weight is placed outside. Any engineer who wishes to keep up rotation by a fly-wheel places the weight round the edge of it; it would not be exactly the same thing to put the weight in the middle and spokes outside.

The Chairman: Mr. Abel, who has had a good deal to do with gun-cotton, will perhaps, be kind enough to tell us what is going on with reference to that substitute for gunpowder. It is important as bearing upon the question of freedom from smoke.

Mr. Abel, F.R.S.: As far as absence of smoke is concerned, gun cotton will carry all before it, but there were so many points to be worked in connection with its application to small arms for military purposes, that although I have been somewhat persevering in working at this subject, and although many others who have studied it and are interested in gun cotton, are very sanguine indeed as to its eventual application to military purposes, still I feel that at the present moment I have nothing that would be sufficiently valuable to bring before the Society with regard to its special application to breech-loading arms, that would warrant my detaining you to-night. I may say, however, that I am most sanguine as to the eventual application of gun-cotton to breech-loading arms for military purposes.

Captain Majendie: When I selected this particular phase of the question this evening, I was in hopes that we should have limited the discussion as much as possible to facts and have avoided theories, because it is impossible in a lecture of this sort to deal with the abstract theories of gunnery, and although it would doubtless be very interesting to discuss questions of gunnery with Captain Selwyn, we

---

* The following additional information has been supplied since the meeting.

All the parts are interchangeable and have a great deal of play. The barrel or breech is choked or rendered gas-proof by the expansion of an ebonite ring or washer. The action of the gas is on a steel disc and not on the ebonite. The weight of the rifle is under 9 lbs., and the bore is ·433 of an inch. The cartridge is made of paper, and is all consumed or passes out at the muzzle. The ignition is in the cap and a needle strikes the composition, after passing through a small elastic washer. The cartridge can be made up by an ordinary soldier, should the regular supply of ammunition fail.—Ed.

cannot do it satisfactorily or usefully here. Therefore, I propose to deal as far as can with the simple questions of fact which have been raised in this discussion. With regard to the calibre, I really am unable to give an answer to General Boileau's question because I do not belong to, nor have I any connection whatever with, the Committee who have to decide upon the introduction of the new arm. There is no doubt a sort of impression prevailing that the arm will certainly be of smaller calibre than the present, and perhaps taking one view and setting it against another view, half an inch may be considered the better calibre. Of course that is a matter of opinion. It is a question for the gun-makers, and it is open to them to submit whatever plans they like.

Now as to the question of accuracy, we have been told that the Snider rifles shoot nowhere, and that the trajectory is anywhere. I can hardly do better than read the last report of General Hay's, which I have with me, in which he says, "I have the honour to report for the information of his Royal Highness that the shooting at 600 yards and at 800 yards is better than that of the muzzle-loading rifle of 1863." That is General Hay's opinion about the shooting of the Snider rifle. On the question of trajectory, I have been carrying out some experiments lately. General Hay gives the trajectory in one of his experiments at about thirteen minutes difference in favour of the muzzle-loading Enfield at 600 yards. Now I make out that at 300 yards the Snider is equal, and at 500 yards—I certainly used different powders on these occasions*—the Snider, instead of being thirteen minutes inferior, was three minutes and five seconds superior to the Enfield rifle. At 800 yards the lightness of the ball began to tell, and we got a difference only of twenty-two minutes as against the Snider rifle. Therefore, I think, if these experiments were repeated—I am quite satisfied my own have been made carefully with fixed mechanical rests—we should probably find, as might reasonably be expected, that at moderate ranges, at 500 or 600 yards, the trajectory of the Snider rifle would be slightly superior, while at 800 yards it would be very little inferior.

The question of penetration has been raised, and I can only say that the penetration of the Snider rifle at fifty yards is only ·3 of an inch below that of the Enfield rifle; that is to say, the ball will pass through 10·8 half inch deal boards, while that of the Enfield rifle passed through 11·4 half inch deal boards. So much for the questions of accuracy, trajectory, and penetration.

Then with regard to the question of cost. Gentlemen seemed to suppose I was comparing the present ammunition with the old 1853 ammunition. That is quite a mistake. I did not intend to compare it with that when I said that the present cartridge is very little superior in first cost to the paper cartridge, I meant the papier mâché breech-loading cartridge. A breech-loading cartridge is necessarily more expensive than a muzzle-loading cartridge; but even if we take the figures as compared with the old Enfield rifle cartridge, I dispute the statement that the difference is £2 per thousand, and say it is not more than 19*s.* 1*d.* a thousand, which will make a large difference in the gross calculation of £80,000. But with regard to the other question of expense, as between different sorts of breech-loading cartridges, this must always be subject to the question of efficiency. I have started by saying that certain conditions must be satisfied in a breech-loading cartridge, and I think there is no gentleman here this evening who would be inclined to dispute it. I made it as wide as I could in order to avoid disputes. When you have satisfied these conditions, not before, then you may begin to think of expense.

---

* The following explanatory note has been forwarded for insertion:—"Different powders having been used at the 300 and 500 yards ranges, the symmetry of the result is spoiled. Theoretically the Snider, having the lighter bullet, should have a lower trajectory at both these ranges; but in this experiment it was lower only at the 500 yards range, but not higher, only equal at the 300 yards range. This apparent discrepancy might seem to establish some error in the experiment or calculation, in absence of the true explanation, viz., that the powders used at the 300 and 500 yards ranges were different."—Ed.

With regard to the copper cartridges, which are of the ordinary type, there are three important things to take into consideration, the weight of the cartridge, the economy of the cartridge, and the extraction of the cartridge. If you make a cartridge very thin, so that it expands very much, you get into difficulties with certain arms in the extraction; if you make it very thick, you get into difficulties of expense or weight. It was stated, I think, in the course of the discussion, that at Woolwich we do not calculate capital or plant and depreciation of plant, or cost of ground, or cost of materials which we use. I may say distinctly, having been five years in the Royal Laboratory, that we charge every one of those things. We charge cost of the ground for the building to stand on, we charge depreciation of the plant, and of the buildings in which the manufacture is carried on. We have a balance-sheet which is based precisely upon ordinary commercial principles, and which will enable us to place our charges in direct comparison with those of any commercial firm.

Captain Selwyn: What profits do you charge?

Captain Majendie: I cannot recollect at this moment the exact figures, but we have a regulated scale of depreciation of plant.

Captain Selwyn: Do you charge any profits, and if so what percentage?

Captain Majendie: We bring out the ordinary cost of the material of the thing which we are producing at ordinary price.

Captain Selwyn: What ought volunteers to be able to get a cartridge for?

Captain Majendie: I believe the price at which we produce the present "Boxer" cartridge, taking all these things into consideration, and taking the cost of powder, which has been calculated on a greatly increased rate lately, is £3 13*s.* 6*d.* per thousand.

A Member: How many grains of powder do you calculate for each cartridge?

Captain Majendie: 70 grains. There was one question about weight. It was stated that we defined the weight for the new competition at 6 lbs. 4 ozs., and that the present cartridge was very much in excess of that weight. Now the weight of sixty rounds of the present ammunition is 6 lbs. 2 ozs., which is 2 oz. within the rate laid down for the competition. Sixty rounds of Enfield ammunition weighs 5 lbs. 11 ozs., while sixty rounds of Snider ammunition does not weigh 7 lbs., as has been stated, but 6 lbs. 2 ozs.

The Chairman: Whatever difference of opinion there may be, and difference of opinion in a meeting of this sort brings out perhaps very valuable discussion, there can be no doubt of this, that the giving of these lectures, whether by any person in authority or by any person who is kind enough to come, who is not in authority, to give us the result of his experience, is most valuable to the Officers both of the Army and the Navy, and to those gentlemen who come here and listen. Therefore I think you will all agree with me that Captain Majendie is entitled to our best thanks for his kindness in giving this lecture.

P.S.—A change has been made in the construction of the ammunition for the Snider rifle, with a view to reducing the cost of production. The quantity of brass in the coiled case has been reduced to 1¼ turns; brown paper has been substituted for white; the base-cup is made of thinner brass; the base-coil is replaced by a thin cup; the base-disc is now made of iron; and a slight alteration has been effected in the bullet, which, although retaining the weight of 480 grains, has now 4 cannelures, and is better adapted for use with the short Enfields and naval rifles, than the pattern of bullet which it replaces.

Ammunition embodying these improvements is distinguished by the numeral V.

9th July, 1867. V. D. M.

Evening Meeting.

Monday, April 1st, 1867.

VICE-ADMIRAL Sir H. J. CODRINGTON, K.C.B., in the Chair.

NAMES of MEMBERS who joined the Institution between the 18th March and 1st April, 1867.

ANNUAL.

Middleton, Francis D., Maj. 29th Reg. £1.
Burgoyne, R. dhu H. G., Lieut. 93rd Highlanders. £1.
Moray, H. E. H. Drummond, Lieut. Sco. Fus. Gds. £1.
Majendie, Vivian D., Capt. R.A. £1.

ON THE ECONOMY OF FUEL, COMPRISING MINERAL OILS.

By Professor W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S., &c.

This paper contains no new principle, nor any new series of experimental researches. It is an attempt to state in a compendious shape the scientific principles that regulate the economy of fuel—a shape in which they may be conveniently applied to any experimental data that now exist, or that may hereafter be obtained.

In the first place I shall have to make a few preliminary remarks on the most convenient way of expressing quantities of heat for such purposes as the present. For purely scientific purposes, the ordinary unit of heat is so much heat as raises by one degree the temperature of the unit of weight of water, whatsoever the unit of weight may be. In British measures, the ordinary unit of heat for scientific purposes is the quantity of heat which raises the temperature of one pound of water by one degree of Fahrenheit's scale—say from the temperature of 39° Fahr. to that of 40° Fahr. When we say that during a certain process—the burning, for example, of one pound of a certain sort of fuel—so many units of heat are produced, we mean that by means of that process, if there be no waste of heat, so many pounds of water have their temperature raised by one degree.

I have here made a memorandum as to the ratio of the British unit and the metrical or French unit, because the employment of French measures for scientific purposes is so common that it is important that their relation to British measures should be borne in mind, especially now that their use is permissive in this country.

One degree centigrade = 1·8 degree Fahr.;
One kilogramme = 2·2046 lb.;
therefore, one French unit of heat = 1·8 × 2·2046 = 3·968 British units.

Connected with this mode of expressing quantities of heat, is the

mode of measuring such quantities in delicate scientific experiments; and that is by the instrument called the Water Calorimeter, where there is a certain mass of water, and a process is performed, such as burning a certain quantity of a combustible substance, and the temperature of that water is raised. Then, knowing how many pounds of water there are in the calorimeter, and knowing how many degrees its temperature has been raised, we can calculate the quantity of heat which that water has received. That is a process which, simple as it may look at first sight, requires great care and delicate apparatus, to ensure accuracy. There is one circumstance that is a fruitful source of inaccuracy, or would be so but for the precautions that are used to avoid it; and that is, the temperature not being uniform throughout the different parts of the mass of water. We have a vessel containing a certain quantity of water; we have a thermometer at one point in that vessel, and that thermometer shows a certain temperature. But the temperature in other parts of the vessel may be quite different. It is necessary, therefore, that the water-calorimeter should be provided with an "agitator," like a screw propeller, for stirring or mixing all the particles of the water intimately together, and for ensuring so constant a circulation in every part of the calorimeter, that the whole of the water shall be at one temperature.

Then there are always certain causes of waste of heat at work. The water in the calorimeter loses heat by conduction, and heat is carried off by the escape of the hot gases produced by the combustion; and the losses of heat which thus occur have to be measured and to be allowed for.

The measurement of heat, then, by the calorimeter is suitable for a scientific laboratory; but on account of the minute precautions that it requires, it can scarcely be considered practicable in such a way as to be trustworthy in experiments on a great scale.

As an instance of the way in which errors may arise in that mode of measuring heat, suppose that we have a certain boiler containing water, and that we apply some heating process to that water until bubbles of steam begin to come from it under the ordinary atmospheric pressure. We should run a risk of serious error if we assumed that the whole mass of water was at the boiling point. The upper stratum of water may be at that temperature, and may be giving out bubbles of steam, but the lower strata of the water may be at lower temperatures; and we may be led to form most erroneous estimates of the quantity of heat given out during the process if we conduct the experiments in such a manner as that.

For practical purposes, connected more especially with the steam-engine, the most convenient unit of heat appears to be the *unit of evaporation;* that is, so much heat as evaporates a unit of weight of water under the mean atmospheric pressure of 14·7 lbs. on the square inch, the water being supplied to the boiler at the same temperature at which it is evaporated; that is to say, at the *standard boiling point* of 212° Fahr., or 100° cent. It is a perfectly definite unit of heat, and one that is easily measured and easily identified.

Then, in certain respects, it is identical in the measures of all

countries whatsoever. There is no question about different standards of weight or measure, when we speak of the number of units of weight of water that are evaporated at the standard boiling-point by a unit of weight of a given sort of fuel. That number is precisely the same whatsoever the system of measures we employ, whether British or French, or the system of any other nation.

The British unit of evaporation is this:—The quantity of heat that evaporates one pound avoirdupois of water at the standard boiling-point of 212° Fahrenheit, or 100° centigrade—the feed-water being supplied to the boiler at the same temperature at which it is evaporated. It is equal to 966 ordinary British units of heat: that is 966 times the quantity of heat that raises the temperature of 1lb. of water 1° Fahrenheit. The French unit of evaporation is the quantity of heat that evaporates one kilogramme of water at the same standard boiling-point, and it is 537 ordinary French units of heat.*

Now, that is a unit of heat specially convenient for purposes connected with the practical employment of fuel. For one thing, it avoids the use of large numbers. That may seem, at first sight, a trifling matter; still, it is of considerable convenience in practical calculations, that we have to deal with units and tens, instead of thousands and tens of thousands. Another advantage is, that we have the heating powers of fuel expressed in terms of the practical effect that fuel is most frequently employed to produce. A third has been already referred to; viz., independence of special systems of weights and measures in expressing the heating powers of fuel.

Connected with the expression of quantities of heat in units of evaporation, is the experimental measurement of such quantities by means of the evaporation of water; and here, too, certain precautions must be observed in order to ensure accuracy. In the first place, the condition that the water shall be "fed," or supplied to the boiler at the boiling-point, must either be exactly fulfilled, or any deviation from it must be ascertained and have its effect allowed for. In practice, the feed-water is almost always supplied at a lower temperature than the boiling-point. Then the water is not evaporated under the standard atmospheric pressure; and a further correction is necessary, depending upon the difference between the actual boiling-point and the standard or atmospheric boiling-point.

With regard to the way in which the temperature of the feed-water and the temperature of the boiling-point affect evaporation, I may refer to the following formula:—

$$\text{E (reduced)} = \text{E (observed)} \times \left\{1 + \frac{T_1 - T_f + 0{\cdot}3\,(T_b - T_1)}{966\text{ F. or }537\text{ C.}}\right\}$$

* It is well known that a certain quantity of heat is equivalent to a certain quantity of mechanical work. For instance, we require to expend 772 foot-pounds of work in friction, in order to raise the temperature of 1 lb. of water 1° Fahr. If we multiply 772 by 966 we get the number of foot-pounds of mechanical work that we have to expend in friction, in order to evaporate 1lb. of water: so that, to evaporate any given weight of water by mechanical work alone, we require to perform as much work as would lift the same weight up to the height expressed by the product; that is, 745,800 feet, or 227,300 metres, nearly. That is the dynamical equivalent of an unit of evaporation.

In the first place, you observe that the formula expresses this :—A certain weight of water has been actually evaporated, the feed water being supplied at some degrees lower than 212° Fahr., and possibly the boiling point being some degress above 212°. I denote that weight by "E (observed)."

Now, in order to express this result in standard units of evaporation, we must reduce it to what the weight evaporated would have been had the feed water been supplied at 212° Fahr., and had the evaporation taken place at that same temperature; a weight denoted by "E reduced." For that purpose we must multiply the observed evaporation by 1 + a certain correction. And that correction is got at in this way. Let T_1 denote the boiling point, 212 Fahr. standard, and T_f the temperature of the feed water. Then $T_1 - T_f$ is the number of degrees by which the temperature of the feed water is below the standard boiling point. Let T_b stand for the actual boiling point; from that we subtract the standard boiling point T_1, and take three-tenths of the difference, viz., $0{\cdot}3\ (T_b - T_1)$. Upon this quantity depends the additional expenditure of heat, owing to the actual boiling point being above the standard boiling point. And having added together those two quantities, the depth of the temperature of the feed below the standard boiling point, and three-tenths of the height of the actual boiling point above the standard boiling point, we divide by 966 if the temperatures are taken on Fahrenheit's scale, or by 537 if on the centigrade scale, and we get a certain quotient, generally a small fraction; we add that to unity, and we get a factor by which we multiply the observed evaporation, and the product is the "reduced evaporation."

If the actual boiling point has not been observed, it may still be calculated from the pressure; because there is a connection between the boiling point and the pressure, and the boiling point corresponding to a given pressure may be ascertained by means of formulæ or tables.

Besides this, great care must be taken that the true weight of water evaporated is ascertained. We must either take care that the boiler does not prime, or we must ascertain the amount of priming and allow for it. Priming is the carrying over of water in the liquid state, in the form of spray or mist, along with the steam. Supposing that in the apparatus used it is impossible to avoid priming, there is only one resource: that is, to use an apparatus which will stop all the liquid water from passing, and let nothing but the dry steam pass, and then condense the dry steam in a separate vessel and weigh it. That is scarcely practicable on a great scale, and in the rough sort of way in which many such experiments must be made; and therefore in such cases we must rely chiefly on taking care either that there is no priming, or, if that be impossible, that it is so small as to be unimportant.

Having explained the precise nature of the unit of heat which I intend to use, I will now speak in the first place of the *total* or *theoretical* evaporative power of a given sort of fuel, that is to say, the number of times its own weight of water that it would evaporate at the standard temperature, if there were absolutely no waste of heating power; and in the second place, of the *effective* or *available*

evaporative power, being what we actually get with such furnaces and boilers as we have at our command.

I may observe that the ratio which the effective evaporative power bears to the total evaporative power is a fraction which we may call *the efficiency of the furnace and boiler*. For shortness, we will call it the *efficiency of the furnace*. If the whole of the heat employed by combustion could be effectively employed at the standard temperature, without any waste whatsoever, we should have an evaporative power represented, say by E. But the actual evaporative power is a smaller quantity, say E′ : then we have the ratio,

$$\text{Efficiency of furnace} = \frac{\text{E}' \text{ (available)}}{\text{E (total)}},$$

which is a fraction less than unity; and the nearer that fraction approaches to unity, the more efficient the furnace and boiler are.

With respect to those two quantities,—total evaporative power, and available evaporative power, it may further be remarked that the total evaporative power of a given sort of fuel depends upon its chemical composition alone; while available evaporative power depends not only upon the chemical composition of the fuel, but upon the construction and mode of management of the apparatus in which it is used, upon the supply of air, and upon a great variety of different circumstances.

With regard to the total evaporative power of different sorts of fuel as distinguished from the available evaporative power, we already possess very full and accurate information. Numerous careful experiments on that subject have been made in scientific laboratories; they have consisted in completely burning small quantities of different combustible substances, and measuring the heat with scientific apparatus of a very accurate kind, and taking special care that no heat escaped measurement. For example, in such experiments it has not always been practicable to prevent hot gases, the products of combustion, from passing away at an elevated temperature. But then the volume of those gases and their temperature could always be measured, and the heat that so passed away by the chimney calculated and allowed for. It is from experiments of that sort that our knowledge of the total or theoretical evaporative power of various combustible substances is derived. Many such experiments were made by Dulong, Despretz, and other chemists and natural philosophers, from which a great deal of valuable information was obtained; but the experiments now chiefly to be relied on are those of Messrs. Favre and Silbermann, published in the *Annales de Chimie et de Physique*, for 1852-3, volumes 34, 36, and 37. Those experiments have furnished us with a body of information which, as to combustible bodies that are of any practical importance, leaves nothing to be desired: that is, as to total or theoretical evaporative power. Our knowledge of available evaporative power is still imperfect: much has been learned, but much still remains to be learned.

I will now make some remarks specially upon the total or theoreti-

cal evaporative power of certain sorts of fuel as ascertained by the experiments to which I have referred.

We may distinguish the combustible substances to which those experiments relate into two classes—elementary substances and compound substances. I have stated the theoretical or total evaporative powers of the only elementary substances that are of any practical importance in this table:—

Element.	Oxygen per unit of weight.	Air per unit of weight.	Units of evaporation.	
(1) Hydrogen gas	8	36	64·2	
(2) Carbon, solid	$2\frac{2}{3}$	12	15·0	
(3) Carbon, solid, with half supply of oxygen	$1\frac{1}{3}$	6	4·5	15·0
(4) Carbon, gaseous, in $2\frac{1}{3}$ parts of carbonic oxide	$1\frac{1}{3}$	6	10·5	
(5) Carbon, pure gaseous (inferred by theory)	$2\frac{2}{3}$	12	21?	

You will observe that there are three columns of figures following the name of the elementary substance. The first expresses the weight of oxygen that is required in order to burn completely an unit of weight of that elementary substance. For example, 1 lb. of hydrogen requires 8 lb. of oxygen for its complete combustion. In order to supply that oxygen there are 36 lb. of air required; and in the second column of figures is stated the weight of air. The third column gives the total or theoretical evaporative power, which in the case of hydrogen is 64·2 times its own weight. It has the greatest evaporative power of all known substances. Then follows carbon. A pound of carbon requires $2\frac{2}{3}$ lb. of oxygen to burn it completely, and to supply that oxygen 12 lb of air are wanted; and the complete combustion of carbon produces heat enough to evaporate 15 times its own weight of water.

I may here observe that carbon exists in various states of aggregation, such as charcoal, coke, plumbago, and diamond; that the experiments of Favre and Silbermann were made upon it in those different states, and that the total heat of combustion differs according to the state of aggregation. The more hard and dense the carbon is, the less is the heat we get by the combustion—the reason evidently being that a certain quantity of that heat is expended in overcoming the attraction of the particles of carbon for each other. For example, diamond does not evaporate so much as 15 times its weight of water, because of the heat required to overcome its own cohesion. That has a bearing upon some other phenomena to which I will presently refer. But the quantity I have here set down is the result obtained from carbon in the ordinary states of charcoal and coke.

The third line in the table refers to the result produced by carbon when burned with only half the full quantity of oxygen. It is known to chemists that carbon combines in two different proportions with

oxygen. One part of carbon by weight combined with one part and one-third of oxygen produces carbonic oxide. Carbonic oxide is itself a combustible gas, and in burning it combines with just as much additional oxygen as it already contains, so as to form carbonic acid. If we have a furnace ill supplied with air, so that the carbon only gets half the quantity of oxygen that it needs in order to form carbonic acid, then we have carbonic oxide as the product. Each pound of carbon takes up $1\frac{1}{3}$ lb. of oxygen, and to supply that oxygen 6 lb. of air are required. The total evaporative power is diminished not merely to one half, but to a great deal less than one half: it is only 4·5, or three-tenths of 15, the total evaporative power with a full supply of oxygen.

If we next take that carbonic oxide, the weight of which will be $2\frac{1}{3}$ lb., namely, 1 of carbon, and $1\frac{1}{3}$ of oxygen, and burn it, it takes an additional $1\frac{1}{3}$ lb. of oxygen to burn it; and we get exactly the quantity of heat necessary to make up the deficiency, namely, $10\frac{1}{2}$. The $4\frac{1}{2}$ units of evaporation during the first process and the $10\frac{1}{2}$ during the second, give 15 in all, making up the whole evaporative power of carbon with a full supply of oxygen.

A conclusion can be drawn from this, which I will now explain. It is to be observed that in both those stages of the combustion of carbon, we have the very same thing happening chemically; we have $1\frac{1}{3}$ lb. of oxygen combining with 1 lb. of carbon. But there is this difference in the two stages. In the first stage, where the carbonic oxide is produced from solid carbon, the solid carbon has to be converted from the solid state to a state of vapour or of gas. In the second stage, we have the carbon already in the state of gas. Hence it appears that the cause of difference between the $4\frac{1}{2}$ units of evaporation due to the first stage, and the $10\frac{1}{2}$ due to the second stage of the combustion, must be, that during the first stage, 6 units of evaporation disappear in transforming the carbon from the solid to the gaseous condition: in other words, the *latent heat of evaporation of carbon is six times that of water*. Thus we arrive at the conclusion, that the total evaporative power of pure gaseous carbon is 21, from which if we subtract 6, the latent heat of evaporation of carbon, there remains 15, the total evaporative power of solid carbon.

I have marked that conclusion as to the total evaporative power of gaseous carbon with a note of interrogation (?), because it is a theoretical inference which has not been directly verified by experiment, but only deduced by reasoning. Still it seems to me that there can be little doubt of its truth.

If we could get pure carbon from any natural source in the gaseous state, we should have 21 units of evaporation by burning it, because we should save the six units that are employed to transform the carbon from the solid to the gaseous state—that is, those six units would be available for the evaporation of water.

So far, then with regard to the total or theoretical evaporative power of the elements of fuel.

Now, as to that of their compounds.

It was at one time supposed, from experiments relied upon at the

time, that on order to calculate the total evaporative power of a given compound, we had only to take the separate elementary substances of which it consisted, calculate the separate evaporative powers due to those constituents, and add them together. But in the present state of knowledge we know that such a result as that is not quite accurate: that we require to subtract whatsoever heat may be necessary to separate the constituents of the compound fuel from each other. There are cases where apparently there is no sensible expenditure of heat for that purpose. But in almost every actual case that we meet with, it is evident that a certain quantity of heat disappears in overcoming the attraction of the constituents of a compound fuel for each other. So that having calculated from our data regarding elementary substances the theoretical evaporative power due to the combination of the elements of a compound fuel with oxygen, we must not stop there, but we must subtract whatsoever heat may have to be expended in order to overcome the affinity of those elementary substances in the compound fuel for each other before they can combine with oxygen.

Having premised that, I may call attention to some results of scientific experiment as to the total evaporative power of certain kinds of compound fuel.

The only compound fuels that are of any practical importance are the compounds of hydrogen and carbon. It appears from the experiments of Favre and Silbermann, to which I have referred, that in the case of marsh-gas, which is a compound of two equivalents of carbon with four of hydrogen, or by weight, three parts of carbon to one of hydrogen, we have the following results :—

Constituents.	Equivs.	Parts by weight.	Oxygen.	Air.	Evaporative power.	
C	2	$\frac{3}{4}$	2	9	11·25	calculated as if solid.
H	4	$\frac{1}{4}$	2	9	16·05	
C_2H_4		1	4	18	27·3	sum
		Evap. power by experiment			24·3	
				Difference	3·0	

Three-fourths of the weight of marsh-gas is carbon; one-fourth is hydrogen. Now if we calculate the evaporative power by taking the constituents separately, we get three-fourths of 15, or 11·25 for the carbon, if solid; one-fourth of 64·2, or 16·05, for the hydrogen; making together 27·3. But the total evaporative power is found by experiment to be 24·3; so there is a disappearance of three units of evaporation, over and above a quantity equal to that which would disappear in evaporating three-fourths of an unit of weight of carbon, viz., $6 \times \frac{3}{4} = 4\frac{1}{2}$. That is evidently due to some affinity between the carbon and the hydrogen, which has to be overcome in order to separate them from each other; and which requires, in order to overcome it, the expenditure of $4\frac{1}{2} + 3 = 7\frac{1}{2}$ units of evaporation.

oxygen. One part of carbon by weight combined with one part and one-third of oxygen produces carbonic oxide. Carbonic oxide is itself a combustible gas, and in burning it combines with just as much additional oxygen as it already contains, so as to form carbonic acid. If we have a furnace ill supplied with air, so that the carbon only gets half the quantity of oxygen that it needs in order to form carbonic acid, then we have carbonic oxide as the product. Each pound of carbon takes up $1\frac{1}{3}$ lb. of oxygen, and to supply that oxygen 6 lb. of air are required. The total evaporative power is diminished not merely to one half, but to a great deal less than one half: it is only 4·5, or three-tenths of 15, the total evaporative power with a full supply of oxygen.

If we next take that carbonic oxide, the weight of which will be $2\frac{1}{3}$ lb., namely, 1 of carbon, and $1\frac{1}{3}$ of oxygen, and burn it, it takes an additional $1\frac{1}{3}$ lb. of oxygen to burn it; and we get exactly the quantity of heat necessary to make up the deficiency, namely, $10\frac{1}{2}$. The $4\frac{1}{2}$ units of evaporation during the first process and the $10\frac{1}{2}$ during the second, give 15 in all, making up the whole evaporative power of carbon with a full supply of oxygen.

A conclusion can be drawn from this, which I will now explain. It is to be observed that in both those stages of the combustion of carbon, we have the very same thing happening chemically; we have $1\frac{1}{3}$ lb. of oxygen combining with 1 lb. of carbon. But there is this difference in the two stages. In the first stage, where the carbonic oxide is produced from solid carbon, the solid carbon has to be converted from the solid state to a state of vapour or of gas. In the second stage, we have the carbon already in the state of gas. Hence it appears that the cause of difference between the $4\frac{1}{2}$ units of evaporation due to the first stage, and the $10\frac{1}{2}$ due to the second stage of the combustion, must be, that during the first stage, 6 units of evaporation disappear in transforming the carbon from the solid to the gaseous condition: in other words, the *latent heat of evaporation of carbon is six times that of water.* Thus we arrive at the conclusion, that the total evaporative power of pure gaseous carbon is 21, from which if we subtract 6, the latent heat of evaporation of carbon, there remains 15, the total evaporative power of solid carbon.

I have marked that conclusion as to the total evaporative power of gaseous carbon with a note of interrogation (?), because it is a theoretical inference which has not been directly verified by experiment, but only deduced by reasoning. Still it seems to me that there can be little doubt of its truth.

If we could get pure carbon from any natural source in the gaseous state, we should have 21 units of evaporation by burning it, because we should save the six units that are employed to transform the carbon from the solid to the gaseous state—that is, those six units would be available for the evaporation of water.

So far, then with regard to the total or theoretical evaporative power of the elements of fuel.

Now, as to that of their compounds.

It was at one time supposed, from experiments relied upon at the

time, that on order to calculate the total evaporative power of a given compound, we had only to take the separate elementary substances of which it consisted, calculate the separate evaporative powers due to those constituents, and add them together. But in the present state of knowledge we know that such a result as that is not quite accurate: that we require to subtract whatsoever heat may be necessary to separate the constituents of the compound fuel from each other. There are cases where apparently there is no sensible expenditure of heat for that purpose. But in almost every actual case that we meet with, it is evident that a certain quantity of heat disappears in overcoming the attraction of the constituents of a compound fuel for each other. So that having calculated from our data regarding elementary substances the theoretical evaporative power due to the combination of the elements of a compound fuel with oxygen, we must not stop there, but we must subtract whatsoever heat may have to be expended in order to overcome the affinity of those elementary substances in the compound fuel for each other before they can combine with oxygen.

Having premised that, I may call attention to some results of scientific experiment as to the total evaporative power of certain kinds of compound fuel.

The only compound fuels that are of any practical importance are the compounds of hydrogen and carbon. It appears from the experiments of Favre and Silbermann, to which I have referred, that in the case of marsh-gas, which is a compound of two equivalents of carbon with four of hydrogen, or by weight, three parts of carbon to one of hydrogen, we have the following results:—

Constituents.	Equivs.	Parts by weight.	Oxygen.	Air.	Evaporative power.	
C	2	$\frac{3}{4}$	2	9	11·25	calculated as if solid.
H	4	$\frac{1}{4}$	2	9	16·05	
C_2H_4		1	4	18	27·3	sum
		Evap. power by experiment			24·3	
				Difference	3·0	

Three-fourths of the weight of marsh-gas is carbon; one-fourth is hydrogen. Now if we calculate the evaporative power by taking the constituents separately, we get three-fourths of 15, or 11·25 for the carbon, if solid; one-fourth of 64·2, or 16·05, for the hydrogen; making together 27·3. But the total evaporative power is found by experiment to be 24·3; so there is a disappearance of three units of evaporation, over and above a quantity equal to that which would disappear in evaporating three-fourths of an unit of weight of carbon, viz., $6 \times \frac{3}{4} = 4\frac{1}{2}$. That is evidently due to some affinity between the carbon and the hydrogen, which has to be overcome in order to separate them from each other; and which requires, in order to overcome it, the expenditure of $4\frac{1}{2} + 3 = 7\frac{1}{2}$ units of evaporation.

Then, in the case of olefiant gas, one of the chief constituents of coal-gas, we have—

Constituents.	Equivs.	Parts by weight.	Oxygen.	Air.	Units of evaporation.	
C	4	$\frac{6}{7}$	2·29	10·30	12·9	calculated as if solid.
H	4	$\frac{1}{7}$	1·14	5·13	9·2	
C_4H_4		1	3·43	15·43	22·1	

Here, calculating the total evaporative power from the constituents as before, we find it 22·1; and that is exactly verified by experiment. Still that very fact is itself a proof of the disappearance of some heat in separating the carbon from the hydrogen; because it has to be recollected that in coal-gas the carbon is gaseous; and if we get no more heat from the carbon in coal-gas than we get from solid carbon, it proves that as much heat disappears in separating carbon from hydrogen in coal-gas as is required to evaporate the same quantity of solid carbon.

That is a fact of much importance, and it simplifies considerably the calculation of the total evaporative power of various hydrocarbons; for the calculation for olefiant gas, which I have given by way of example, is not a solitary instance. By applying a similar method of calculation to the experiments upon various compounds of hydrogen and carbon, it would appear that the expenditure of heat in separating the carbon from the hydrogen is, at all events, to a very close approximation, the same as that required to convert the carbon from the solid to the gaseous state. Hence we get for all practical purposes a good approximation to the evaporative power of any such compound except marsh-gas, by taking the evaporative power of each unit of carbon in the compound as 15, and of each unit of hydrogen as 64, and then adding the results together. That has been verified for an immense variety of compounds. It does not hold with mathematical precision, but it holds with a degree of precision that is enough for any practical purpose.

Now as to the effect of the presence of oxygen in fuel. It was established some time ago by the researches of Dr. Joule, to whom science is immensely indebted for many discoveries of a similar kind, that a certain and definite quantity of heat is produced by the union of two chemical elements, and that precisely the same quantity of heat disappears in the separating of those elements. He verified that in a great number of cases, and established it as an universal law of nature. It holds in many other processes besides chemical combinations and decompositions, whether we are speaking of heat or of any other form of physical energy, that whatsoever quantity of energy we obtain by a given process, if we exactly reverse that process we have to expend precisely the same quantity of energy again.

If 8 lb. of oxygen combine with 1 lb. of hydrogen, they produce heat enough to evaporate 64 lb. of water. If we decompose that water by any contrivance, whether by the use of a galvanic battery, by

the superior affinity of carbon for oxygen, or by any other process, precisely the same quantity of heat disappears in overcoming the attraction of the hydrogen and oxygen for each other. Now if oxygen enters into the chemical composition of any fuel, we have this result: calculate how much hydrogen that oxygen requires in order to form water—that is to say, one-eighth part of the weight of oxygen; then the oxygen present in the fuel renders just that quantity of hydrogen unavailable for the production of heat, and takes away from the total evaporative power 64 times the weight of the hydrogen so rendered unavailable. That being known, we have the following rule in the shape of a formula:—

$$E = 15\,C + 64\,H - 8\,O.$$

This, then, is the rule for calculating the theoretical evaporative power of any sort of fuel whose combustible materials are carbon and hydrogen from its chemical analysis. Distinguish the constituents into carbon, hydrogen, oxygen, and refuse. The refuse does harm as being a useless weight, but it does not take away from the heating power of the constituent parts that remain. For every unit of carbon that remains in the compound, we have 15 units of evaporation; and for every unit of hydrogen, in the absence of oxygen chemically combined with it, 64 units of evaporation; and if there is oxygen in the compound fuel, we have to subtract eight times the weight of that oxygen, because the oxygen renders inoperative one-eighth of its weight of hydrogen; and one-eighth of 64 is 8. The remainder gives the total evaporative power of that fuel.

The next formula shows the amount of air necessary in order to supply oxygen for the complete combustion of a compound fuel. It consists of 12 units of weight of air for each unit of carbon, and 36 for each unit of hydrogen, deducting $4\frac{1}{2}$ for each unit of oxygen in the compound; or in symbols,

$$A = 12\,C + 36\,H - 4\tfrac{1}{2}\,O.$$

In an actual furnace it is seldom sufficient to supply just the quantity of air that contains the oxygen required for complete combustion. It is in general necessary to supply a surplus of air in order to dilute and sweep away the *burnt gas*, as we may call it, and to ensure that every particle of fuel shall have air brought in contact with it. In furnaces with a draught produced by a chimney in the common way, the practical result is, that we have to double the quantity of air, or nearly so. But when we produce a draught by a blast pipe, by steam jets, by a fan, or by other means, which thoroughly mix the air with the gaseous fuel, we reduce the surplus air required very much. In some cases we need only half; in some cases we need no surplus at all. Hence the quantity of air actually supplied, ranges from once to twice the quantity necessary for the oxidation of the fuel.

Here follows a table of examples of the total evaporative powers

of some kinds of fuel, as calculated from their chemical composition:—

	C.	H.	O.	A.	E.	Evap. due to C.	Evap. due to $H.-\frac{O}{8}$
Charcoal......	·93	0	0	11·5	14·0	14·0	0
Coke	·88	0	0	10·6	13·2	13·2	0
$C_{18}H_{20}$	·84	·16	0	15·75	22·7	12.7	10·0
$C_{26}H_{28}$	·85	·15	0	15·65	22·5	12·66	9·84
Coal	·87	·05	·04	12·1	15·9	13·05	2·85
„	·85	·05	·06	11·7	15·5	12·75	2·75
„	·75	·05	·05	10·6	14·1	11·25	2·85
Peat, dry	·56	·06	·31	7·7	10·0	8·5	1·5
Wood, dry ..	·50	·05	·40	6·0	7·5	7·5	0

The first line refers to charcoal; the second to coke of average quality; the third and fourth lines give two examples of hydrocarbons, which comprise between them the chief ingredients of rock-oil. In the third, fourth, and fifth columns are stated the proportions of the chemical constituents, ranging, for the rock oils, between 84 per cent. of carbon to 16 of hydrogen, and 85 per cent. of carbon to 15 of hydrogen. The fifth column gives the net weights of air required for the oxidation of the fuel. In the sixth column the theoretical evaporative powers are set down: for rock-oils they are from 22·7 to 22·5, or, in round numbers, we may say 22½. In two additional columns are some figures to show how much evaporation is due to carbon and how much to hydrogen. In rock-oils, those quantities are 12½ due to the carbon, and 10 to the hydrogen, out of the 22½ units of evaporation. Lines 5, 6, and 7 give a few examples of coal. These might be immensely multiplied; but I have given only three specimens. They differ from the kinds of fuel previously mentioned, in having some oxygen in them, which somewhat lessens the evaporative power. Here, too, the units of evaporation due to carbon and to hydrogen are distinguished; for example, line 6 shows 12¾ units of evaporation due to carbon, and 2¾ due to hydrogen. Then follow some results for peat and for wood, on which I need not enlarge.

It is to be observed that the examples given in this table, which are only a few selections out of much more voluminous data, are all taken from good specimens. In fact, it is impossible to found experiments upon bad specimens of fuel, because people do not send bad specimens of fuel to be experimented upon. But it may be taken as the result of practical experience that the evaporative power of bad specimens of fuel from a particular district is about two-thirds that of good. This applies particularly to coal. The difference is chiefly due to refuse or ash. But it does not apply to mineral oils, because they can hardly contain anything else but combustible matter. They do not contain ash; they do not contain earthy matter. The statement then, about qualities of fuel which have only two-thirds of the total evaporative power of those set down in the table, applies to coal and peat; but it does not apply to mineral oils.

I have gone at some length into the principles of the total evaporative power of different sorts of fuel, because it is of great practical

importance to understand them. They show the theoretical limit towards which practice approaches in the course of improvement, but which practice cannot surpass nor even attain; the knowledge of those principles prevents people, on the one hand, from forming too small an estimate of the results which may be got from the economical use of fuel; and on the other hand from indulging in exaggerated estimates of those results. It shows us, indeed, what is the actual waste, when we know the result attained in practice; it enables us to judge how far that result falls short of theoretical perfection, and what room there is for further economy by means of improvements.

Now, as to the causes which make the available evaporative power fall short of the total evaporative power.

In the first place, there is imperfect combustion, all the combustible constituents not being combined with the proper quantity of oxygen. This generally arises either from want of a sufficient quantity of air, or from want of a free mixture of air; and in the case of gaseous fuel it sometimes arises from sudden cooling, through coming in contact with some cold substance, so as to lower the temperature and extinguish the flame before the combustion is complete. As to the want of a sufficient quantity of air, the remedy is obvious enough. It is to take care that there are openings enough to admit a proper quantity of air. Still we may supply a very large quantity of air, yet fail to realise complete combustion, because of the air not being thoroughly mixed with the fuel. I have already referred to what we have to do when we have a draught produced, in a quiet way, so to speak, by the ordinary action of a chimney. There is not sufficient agitation of the air to produce a thorough mixture, unless we introduce a surplus quantity of air. We may produce forcibly a thorough mixture of air with the gaseous fuel by such means as the use of the blast pipe, the steam jet, or the fan. This is one of the chief uses of the blast pipe in locomotive engines. The object of blowing steam jets into the furnace, or, at all events, one of those objects, is to produce the same effect. Although the fuel should be solid coke, a great deal of it is actually burned in the gaseous state; for it first undergoes that sort of half combustion that produces carbonic oxide. That carbonic oxide is a gaseous fuel; and in order to burn it completely we have to make sure that it is thoroughly mixed with air. Then, the complete combustion of fuel that is in the liquid state may be promoted by contrivances that blow it into spray, or convert it into vapour, and in that condition mix it with the air. We may blow it into spray by the impulse of a jet of steam; or by superheating that steam we may at the same time convert the liquid fuel into vapour, and mix that vapour with air.

As to the extinction of the flame by cooling, that is a thing which often takes place in steam-boilers, heated by coal that contains a large proportion of volatile constituents. When first set free and brought in contact with the air, these take fire; but the flame comes in contact with the heating surface of the boiler, which is at a comparatively low temperature; the combustion is checked, and instead of combustion, we have decomposition of the hydrocarbon gas. The carbon and hydrogen

are separated—the carbon in the shape of black particles, which form smoke; and both the carbon and the hydrogen go off to a great extent unburnt.

I might here enter into a calculation to show what amount of waste of fuel may arise from an imperfect or ill-managed supply of air. It would perhaps occupy too much time to go into detail; but I may refer to a fact which I have already stated in the case of carbon, that if it combines with only half of the quantity of oxygen that it requires for complete combustion, we have only $4\frac{1}{2}$ units of evaporation, instead of 15. The evaporative power is reduced to three-tenths, so that seven-tenths of it are lost.

Then in the case of fuel like bituminous coal, which consists partly of solid carbon, and partly of volatile compounds of carbon and hydrogen, we may have the hydrogen going off imperfectly consumed, either alone, or carrying away more or less unconsumed carbon along with it. From what I have stated respecting how much of the evaporative power of the bituminous coals depends on the carbon, and how much on the hydrogen, it will be understood how much may be lost in different cases: $2\frac{3}{4}$, or in some cases 3 units of evaporation if the hydrogen alone goes off without being consumed; and if it carries off a certain amount of carbon along with it, that waste may be more than doubled.

I have spoken of the remedies for the imperfect combustion of gaseous fuel, by thoroughly mixing it with the air, either by a blast pipe or by a steam jet. As to the thorough combustion of carbon in the solid state, one way to ensure that is, by keeping a thin fire, that is to say, having a very large grate in proportion to the quantity of coal to be burned, and burning only a small quantity of coal, say 10 lb. per hour, on a square foot of grate. I have known as little as 4 lb. per hour burned on a square foot of grate. Thus we may ensure that there shall be free access of air to all the burning carbon. But another plan is, to let the combustion on the grate be imperfect, by keeping a thick layer of coal on the grate, which turns into coke, and then to introduce a supply of air above the level of the fuel on the grate, for the purpose of consuming the carbonic oxide and the hydro-carbon gas that are in the gaseous state. There is a known contrivance, called the "dead plate." It is a cast-iron plate at the mouth of the furnace, on which the fresh coal is laid and exposed to a heat sufficient to distil away the volatile constituents. The coal gas so distilled passes over the layer of red-hot coke on the grate: a surplus of air passes through the red-hot coke, sufficient to burn the coal gas. Then, after the coal gas has been all distilled away and burned in that manner, the coke that remains on the dead plate is pushed on to the grate, to be burned in its turn.

In the case, then, of burning coal which is a compound of fixed carbon and volatile hydrocarbon, we have the two plans, of a large grate and a thin bed of fuel, where we have plenty of space; or, of a comparatively small grate and a thick bed of fuel, and introducing air above the fuel to burn the gas, where the space is limited.

There are numerous contrivances for the efficient combustion of coal

which cannot now be described in detail, such as the use of a separate combustion-chamber of brick, in which the burning of the whole constituents of the fuel, solid and gaseous, is completed, the chamber being built with double walls of brick, and a double arch of brick over the top, so as to prevent loss by conduction. Another plan is to make what we may call "crude gas," by the imperfect combustion of coal, so as to turn all the combustible constituents into a mixture of coal gas and carbonic oxide, and then to burn that mixed gas. There are also double furnaces, air-valves of various kinds and in various places, and an endless series of other inventions.

So much, then, as to the waste of heat through imperfect combustion, and the means of preventing it.

There is another sort of waste of heat: that is, by conduction and radiation. It was ascertained by Péclet, that about half the heat that comes from coal in a furnace radiates directly from the incandescent fuel, and is not carried by the current of flame, or the hot gas, to distant parts of the heating surface. We must be careful that none of that heat escapes from the furnace. For example, if there are openings in the furnace door to admit air, we must take care that the door at all events is double, with the openings of the inner plate not directly opposite those of the outer plate, so that radiation shall not take place through them. Or, we may have a grating of several plates placed edgewise close together, between which the entering air may pass, and so absorb the heat that has been radiated to those plates. I have already referred to fire brick casings round a combustion chamber to prevent loss by conduction. The prevention of that sort of loss is a very obvious matter.

I now come to a third mode of waste of heat, which is a highly important one: that is, the waste of heat up the chimney. The products of combustion escape by the chimney at a temperature above that of the external air. Whatsoever heat is expended in raising the temperature of those escaping gases above the temperature of the external air, is a total loss as regards evaporative power. It is useful for certain purposes. It is useful to rarefy the air in the chimney, and to make it lighter than the air outside, and so to cause the draught of the furnace; but it is not useful as regards evaporative power. The quantity of heat wasted in this way depends upon the specific heat of the burnt gas, which is about one-fourth of that of water; so that for every pound of gas that goes up the chimney, and for every degree of elevation of the temperature of that pound, we have one-fourth of an ordinary unit of heat expended: being in round numbers and for Fahrenheit's scale, about the $\frac{1}{4000}$th part of an unit of evaporation per degree and per pound of burnt gas.

The following formula expresses in a condensed form the waste of heat up the chimney in units of evaporation:—

$$\text{Loss by Chimney} = \frac{1 + A'}{4000}.\ T_c\ \text{Fahr};$$

and the following are examples :—

1 + A′	13	19	25
Tc	600	600	600
Vol. gas, cubic feet ..	325	475	625
Loss of evap. power .	1·95	2·85	3·75

1 + A′ denotes the weight of burnt gas per unit of weight of fuel, and Tc the elevation of the temperature of the chimney, in degrees of Fahrenheit, above that of the external air.

The products of combustion, for the reasons I have already stated, are more or less diluted with additional air; so that in the case of ordinary bituminous coal, 1 + A′ ranges from 13 to 25, and 19 is an intermediate value. In the case of hydrocarbon oils, the air that is required for oxidation is about 15·3. We have to add the weight of fuel, and we shall get about 16·3 for the net weight of burnt gas; and it is quite possible that in a well-contrived furnace, no surplus air may be necessary. We multiply the weight of burnt gas which goes up the chimney by the elevation of temperature in the chimney, and if that temperature is expressed in degrees of Fahrenheit, we divide by 4,000, and the quotient is the evaporative power lost up the chimney.

It is easy to see that in order to diminish that sort of loss, we must in the first place have the temperature of the chimney no higher than is absolutely necessary. When the chimney is the means of producing draught, it can be demonstrated that the most effective draught is produced by a temperature not much above 600° Fahr.

It is still more important to avoid an unnecessarily large supply of air to the fuel. We must supply the quantity of air that will give the oxygen necessary to consume the combustible elements, otherwise we shall have enormous loss from imperfect combustion. Every additional pound of air we supply carries to waste a certain amount of heat. I have said that when we produce the draught by means of the rarefaction of the air in the chimney alone, we require 12 lb. of air per lb. of fuel for oxidation, and 12 lb. more for dilution, making altogether double the net quantity of air. If we use the blast pipe as in a locomotive, I think it is quite certain that it is not necessary to supply more than once and a half the net quantity of air, and in some cases we need only supply a mere fraction more than the net quantity. I have very little doubt that in methods of burning mineral oils which I have seen tried, where by the help of a jet of superheated steam, the fuel is thoroughly mixed with the air immediately before the process of combustion, the net quantity of air is sufficient of itself, and we may altogether avoid that very great waste of heat which arises from the supply of an additional quantity of air.

To exemplify the waste of evaporative power that may so arise, I have annexed a few calculations to the formula. I have taken the weight of the gas going up the chimney as first 13 times, then 19 times, then 25 times the weight of the fuel burnt. I have taken the temperature of the chimney as 600° in each case, that being the best temperature when we use chimney draught. In the next line are stated the volumes of gas that go up the chimney in cubic feet per pound of fuel. That,

however, is a collateral matter. In the next line is stated the loss of evaporative power due to the escape of that gas. And observe that it is in very favourable circumstances. The temperature of the chimney is no higher than is necessary to produce the best draught, yet in the first column the loss of evaporative power is 1·95; in the second, 2·85; and in the third it is 3·75. Losses much exceeding those are very common.

If the draught is produced by artificial means, such as a steam jet, we do not need rarefaction of the air in the chimney in order to produce a draught. We may have the rarefaction as an auxiliary, but it is not indispensably necessary. In fact, it seems that if we adopt proper means for producing draught by mechanical appliances, we may use such a heating surface as to take up almost all the heat of combustion, and reduce the loss of heat by the chimney to an inappreciable quantity.

I will now say something regarding the results deduced from a comparison of experiments by many authorities upon the effect of extent of heating surface upon the economy of heat. It is through the heating surface that heat is communicated from the flame and hot gases to the water in the boiler. Upon the extent of the surface depends very much the proportion of that heat which is made available. It is not yet possible to lay down any precise theory upon this subject. But the formula I have written down gives, at all events, a good rough approximation to the results of experiment and of practice in a variety of different cases:—

$$\frac{E'}{E} = \frac{bS}{S + aF}$$

Values of a		·5	·3, &c.
,, b	1·0	·93	·9, &c.

It is based upon principles of this kind,—that whatsoever parts of the heating surface are nearer to the fire, take proportionally the greater shares of the heat, and that each additional area more and more distant from the fire takes proportionally less and less heat. Suppose a certain area of heating surface in immediate contact with the furnace: it will absorb a certain quantity of heat. Then, the next area of equal extent beyond that will absorb a much less quantity of heat; and so on, each successive additional area absorbing a less and less quantity until at length it becomes no longer practically advantageous to increase the area of the heating surface.

By comparison of a good many experiments and a good many results of practical experience, we have this approximation to the proportions of the available to the total evaporative power of fuel, as depending upon heating surface. Let S denote the number of square feet area of heating surface, and let F denote the number of pounds of fuel burned per hour; let a and b be co-efficients to be deduced from experience. We multiply the area of heating surface by the co-efficient b, and we divide by the area of heating surface, plus the number of pounds of fuel burned per hour, multiplied by another co-efficient a.

From the practical results to which I have referred, it would seem that the value of the co-efficient a, upon which the waste of heat mainly depends, ranges between 0·5 and 0·3. It is 0·5 for a chimney draught, and it is 0·3 for the draught produced by a blast pipe or jet of steam. This 0·3 is found as the result of the use of the blast pipe in locomotives. And it would seem that the co-efficient a is very nearly proportional to the square of the ratio of the total weight of burnt gas to the weight of fuel; so that if we double the quantity of burnt gas, per pound of fuel, we shall quadruple this co-efficient a; if we can reduce the quantity of gas, per pound of fuel, to one-half, we shall reduce the co-efficient a to one-fourth. As to the value of b, it comes very near to unity in some cases, in which care has been taken to make the motion of the water in the boiler take place in the most advantageous direction, relatively to the motion of the products of combustion, that is to say, in the reverse direction—to introduce the feed water in contact with the coolest part of the heating surface, and let it advance gradually to the hottest part, where it is finally converted into steam. That principle was carefully attended to in the boiler invented by Lord Dundonald. If that precaution is neglected, we may lose from 8 per cent. to 10 per cent. of efficiency. But the diminution of efficiency depends mainly upon the waste of heat up the chimney, which is brought into the formula and provided for by the co-efficient marked a.

Here is an example of a particular case:—

$$\text{Let } \frac{S}{F} = 1 ;\ a = \cdot 5 ;\ b = \cdot 9 ; \text{ then } \frac{E'}{E} = \cdot 6.$$

We will suppose that we have one foot of heating surface for each pound of fuel burned per hour: then S divided by F is equal to 1; that we use a well managed chimney draught, so as to have a equal to 0·5; that we do not pay particular attention to the place where we introduce the feed water, and do not use a feed water heater; so that we get b equal to 0·9. Then, by introducing those values into the formula we get for the efficiency, or proportion of the total evaporative power to the available evaporative power, 0·6. This is a very common case in practice, and when it is realized it is considered a very good result. If we have a specimen of coal whose total or theoretical evaporative power is 15, and we get 6 per cent. of that, and its actual or available evaporative power is 9, that is regarded as a good performance. You see how that arises,—how step by step such a result is arrived at upon scientific principles.

I may observe that the actual efficiency of furnaces—the proportions of the available to the total evaporative power of fuel, go through a very wide range. We find cases in which there is only 45 or 50 per cent. of the total evaporative power actually realized, or made available, and yet these are not considered very bad. From 55 to 60 per cent. are ordinary results, in good marine boilers. When we get above those we get to results that are rather uncommon, and when we get to 80 or 90 per cent. being realized, it is considered very extraordinary.

In conclusion, I may make the following observations on what we may look forward to as the probable result of the introduction of such classes of fuel as mineral oil as substitutes for coal. Coal is a very complex kind of fuel. To ensure the best possible economy in the use of it requires the fulfilment of many different conditions, some of which conflict with each other. We have to burn fixed carbon, and we have to burn the gas that is disengaged from the coal. We may burn one very efficiently, and not burn the other. It is extremely difficult to manage the introduction of air, so that there shall at once be no risk of a deficiency of air, which causes imperfect combustion, and gives bad economy in one way; or a surplus of air, which carries heat to waste up the chimney, and causes bad economy in another way. On the one hand we are exposed to the risk, from any little fault of construction or management of the furnaces, of the hydrogen going off unburned, and of its carrying off a large portion of the carbon unburned also. On the other hand we are exposed to the risk of solid carbon being imperfectly burned, and going off as carbonic oxide. The contrivances for diminishing the causes of waste are somewhat difficult and complex to apply in practice; and above all, too much depends upon the skill of the fireman or stoker. We may say almost everything depends upon the way the furnace is managed. The very best furnaces, the very best boilers that were ever contrived, may be made extremely wasteful by a careless stoker. On the other hand, in using mineral oils, we are somewhat in the position of the chemist who has got a good burner for burning coal gas. We have to contrive a suitable apparatus for introducing that oil into the furnace in such a way that it shall be thoroughly mixed with air—whether in the state of vapour, or in the state of fine spray, with or without the assistance of some porous substance to act as a wick. A steam jet seems to be the most efficient apparatus for that purpose. Then if our apparatus is properly contrived and properly constructed, and works in the right manner and produces complete combustion at first, there is no reason to suppose that it will ever act badly if treated with ordinary care.

It would seem to be no difficult matter with fuel of that sort to diminish the waste of heat through imperfect combustion to nothing, and the waste of heat through hot gases going up the chimney to something very small indeed. In fact, such an efficiency as 90 per cent. of the total evaporative power being realised, instead of being a very extraordinary thing, may be looked for as a very ordinary thing; so that, taking the theoretical evaporative power of some hydrocarbon compounds at $22\frac{1}{2}$, we ought not to be surprised that even with rude apparatus, in a merely experimental state, we get an evaporation of 19 or 20 times the weight of the fuel—say 19, at all events. I believe that has been realised in experiments that have been made, and that are now being continued.

There is another phenomenon as to which I may say something—the mode of operation of steam jets in cases of this kind. There can be no doubt that one use of the steam jet is the mechanical action to which I have frequently referred—the thorough mixing of the gaseous

fuel with the oxygen required for the purpose of combustion—the churning of the particles all together, so that they shall be brought thoroughly into contact. There may be another mode of operation of steam—a chemical action whose effect is to prevent the deposition of unconsumed carbon from gaseous fuel. All hydrocarbons have a tendency when raised to a high temperature to deposit the carbon, unless it be instantly burned. We see that in the smoke that a flame tends to give out, if there is anything to prevent immediate and free access of oxygen to the carbon. Now it is possible that besides its mechanical action, the steam jet may also have a chemical action of the following kind:—The oxygen of the steam combines with the carbon of the hydrocarbon fuel, and the hydrogen of both is set free; there is a mixture thus produced of carbonic oxide and of hydrogen, which is sure to be completely burnt as soon as a sufficient quantity of air gets access to it, and thus the deposit of unconsumed carbon is entirely prevented.

I have now gone as far as I think I can venture to occupy the time of this meeting. It would be out of place for me to enter upon the consideration of nautical questions, such as the comparative weight and bulk of different sorts of fuel; therefore I have now only to thank you for the kind attention with which I have been heard.

The CHAIRMAN: Does any gentleman wish to make any remarks upon the paper?

Captain JASPER SELWYN, R.N.: This is a subject which has engaged my attention for the past two years. I had the pleasure of bringing it to the notice of this Institution, persuaded as I was of the enormous value of the whole question, more particularly to the Navy of this country. I knew that if the economy which Professor Rankine has put before us, and the use of materials better adapted for such combustion as we require, were once brought into play, that a very great proportion of the unjust fears, which existed two years ago very strongly indeed, as to the consumption of our whole coal supply, would be met by an economy which was the truest method of supplementing any possible failure; though I scarcely believe such to exist. I am sure that had the members of this Institution been more generally aware, particularly my naval brethren, of the very complete way in which the whole question of the economy of fuel would have been brought before them to-night, we should have seen a very much larger attendance than we now see. However, it matters the less, because this is a paper which will be read with the greatest attention in the Journal. It will, I am persuaded, command a very wide class of readers, not alone in the Navy, but among all those who are interested in this great question; among foreigners as well as our own countrymen. Professor Rankine has followed up, most beautifully, I think, the composition of fuel, the value of each proportionate part of it in accomplishing the effect we desire, the causes of loss, and the possibility of economy. The points to which he has drawn your attention are those which belong to the science which he has cultivated with so much success. The points to which I had the honour of drawing your attention were principally those which affect our own profession, inasmuch as this was a fuel which alone could be burned with greater economy, would require fewer attendants, economising their wages and victuals, would be attended with more certainty even by inexperienced persons, and would give us a power of stowing and taking it on board conveniently in places where we could not have so stowed it formerly, and with none of those wretched residuary products which we find so much difficulty and so much nuisance in getting rid of. The Professor has adverted to the causes of loss in the deposit of carbon. I think I may ask him whether he is not of opinion that that has another and most important effect, which is that the heating surfaces, becoming covered with such a

deposit, lose very materially their efficiency; and that wherever an entire absence of such deposit can be obtained, it not alone indicates economy of the unconsumed carbon which is now burned, but it indicates also the increased efficiency of the heating surfaces, which are left in their integrity, as the maker intended them to exist. The question also of blasts has been brought very strongly before you, showing you the economy of the artificial blast, and where this blast is supplied, at such a point of the furnace as to be itself a contributor to the total heat of combustion, and in this way it becomes clearly very much more valuable. I think I gathered from what Professor Rankine said, that he considered it quite probable that a double decomposition took place, and that the oxygen and hydrogen of the steam contributed to the total heat of combustion; therefore, although it was necessary for the purpose of the blast to sacrifice a certain portion of the steam generated in the boiler, yet that steam, having done a certain portion of mechanical duty in creating blast, also restored to the boiler a certain proportion of the duty of combustion, and was not, therefore, so great a loss as some persons would be led to believe. I put this more in the shape of a question, which I hope Professor Rankine will remember in his reply, in order that my naval brethren may be thoroughly aware of what the bearings of the question are upon points which are perhaps a little obscure. The question of burning mineral oils is also very materially a question of price. Now, if we were to bring to this Institution a proposition to burn something which cost £10, £15, £20 a ton, you would probably meet the person who brought forward such a proposition by saying, "We are aware that such great results may be obtained by such expensive fuel, but we think the expense too great to be justified." But we do not do that here. The term "mineral oil" is applied to a wide range of products, many of them refuse products, such products as arise from the distillation of illuminating oils, and from the refuse of gas manufactories, which are known by the name of "dead earth oils," and which are sold entirely as refuse at prices not very materially above that of coke. From a halfpenny to three-halfpence a gallon, I think, is the price at which they are being actually burned at this moment, and doing duty rather in excess, I think, of what Professor Rankine has claimed for them as yet; not in excess of the theoretical evaporative power, but in excess of the power which Professor Rankine claims in his last words—the theoretical evaporative power very largely secured with very little waste; and an economy in all other points, such as he has spoken of being pretty fairly carried out in these early experiments, has resulted in the duty, done by 30 lb. or three gallons of this dead earth oil at from one halfpenny to three-halfpence a gallon, being precisely the same as had been previously done in the same boiler, with very little change in it, by 60 lbs. of coal. Now, I hope that this subject will not be allowed to drop; that it will be made the subject of very close study indeed by those who are responsible for the cheap and efficient conduct of our naval affairs. I, for one, have to lament the knowledge which I have recently acquired, viz., that one of our most recently built iron-clads can only carry four days' coal to sea. I say distinctly, and I am sure my naval brethren will say with me, that it is utterly impossible we can go on in that way; that to encounter a great European war with such means of locomotive power, even though we should have carried out the views of seamen, and not of those who advocate fighting machines without masts or sails, although we should have carried into effect good sailing ships as well as good steam-ships; yet if they can only steam four days, we shall lose the empire of the ocean for want of being able to go to sea under steam, or, I should say, to keep at sea under steam. I hope, then, that some other person will kick the ball again; that this will not be a subject with respect to which it will be said, "There has been a splendid paper read upon it; we will now consider the subject as closed." Let us rather consider it as only now opened—most ably opened by Professor Rankine. I am sure he has given those who are inclined to study the subject at all the materials with which they may do so successfully.

Mr. J. Richardson, C.E.: As regards fuel, I think we may safely say that that fuel is the most valuable which can be burned with the least waste. We all know that the best coal when carefully burned will evaporate 10 lbs. of water to 1 lb. of coal. Laboratory practice is hardly a test of the result in real practice. In this, evaporation is seldom obtained of more than 7 lbs. of water to 1 lb. of coal. There

is a cause which considerably accounts for this difference. It is necessary in large furnaces, every seven or eight hours, to remove the clinkers from the fire bars; and in doing so, the stokers seldom stop at two-thirds of the contents of the grate, but they rake out nearly all the contents, clinkers and ashes, and the whole go over the side into the sea. This is not enough: every three or four hours all the ashes are raked out, and they go over the side. Besides the waste of coal in raking out the fires; there is also deterioration of coal in hot climates. I have had the opportunity, when at Woolwich, of speaking to engineers there, and they all confess that the amount of evaporation obtained from coke and coal in a long voyage is never more than 6 lb. of water to a pound of coal. Liquid hydrocarbon can be burned nearly without any waste. A ship starting from Southampton, and going to Calcutta, has only got to get her steam up before she leaves the harbour. She may have her furnace doors locked, and she may keep them locked till she gets to Calcutta; and there may be an evaporation obtained of 16 lb. to 18 lb. of water from one pound of fuel, without the slightest change, the whole distance. Evaporation can be obtained without the least trouble to the stokers; they have only to attend to the fire, and see that it does not make too much head, or get on too fast. The fact is that 200 lb. of this liquid hydrocarbon will produce as much heat as 600 lb. of coal. I think this is a most important result in favour of these fuels. There is one fuel that has not yet been tried. This is a solid fuel—naphthaline—and I believe is stronger than creosote. I believe it would produce 24 lb. of evaporation. The Admiralty have given a common service boiler, which is going to be tried, first with coal, and then with the liquid hydrocarbon fuels.

Captain Burgess: What is the price of the naphthaline?

Mr. Richardson: About 18*s*. a ton.

The Chairman: What effect has oxygen when chemically combined with fuel?

Professor Rankine: With respect to the question you have just put, we know from the results of experiment, when oxygen does exist in a state of chemical combination with fuel, that it actually forms water with part of the hydrogen. Its operation is this: it annuls the heating effect of a certain portion of the hydrogen. Suppose in 100 lb. of a certain sort of fuel there are, we will say, 5 lb. of hydrogen, and 4 lb. of oxygen, those 4 lb. of oxygen render ½ lb. of hydrogen inoperative. It does not burn, because it has combined with the oxygen, and it cannot combine with any more. So, while the total hydrogen in the fuel forms 5 per cent. of its weight, the effective hydrogen is only 4½, the remaining portion being rendered inoperative, owing to the presence of the oxygen with which it is able to combine. That seems to be the explanation of the effect of the presence of oxygen in coal. It is well known that it deteriorates the coal. It is further certain that this is one of the causes why coal exposed to the atmosphere becomes deteriorated, and loses heating power. It is equivalent to a partial combustion, leaving so much the less to be burned. Then, with regard to the previous remarks, there is no doubt that all that has been said about the stirring of the fire to remove the clinkers is perfectly sound and perfectly applicable. It is a cause of great waste of heat; and it is important, as regards one objection to the use of coal to which I have already referred, that it leaves so much dependent upon the skill and care of the stoker. There is no doubt that, with very careful firing, the waste from this cause may be reduced perhaps to only about 2½ per cent. of the total evaporative power; but with bad and careless firing, there is no saying how much it may amount to. With mineral oils you are free from dependence upon uncertain matters of that sort. Captain Selwyn put a question regarding the chemical action of a steam jet. There can be no question at all about the advantage of its mechanical action, mixing the air and the fuel together, and bringing the oxygen into thorough contact with the inflammable matter. As to chemical action, as far as chemical action upon steam itself is concerned, it would just amount to this—that the steam is first decomposed into oxygen and hydrogen; and that then the hydrogen is combined with the oxygen again. The result of that, as regards heating power, would be simply nothing. A certain quantity of heat disappears in decomposing water; precisely the same quantity of heat is reproduced by the oxygen combining with the hydrogen again. It is indirectly conducive to a very important saving: all that oxygen which is separated from the

hydrogen during the decomposition of the steam serving to hold in solution the carbon of the inflammable gas or oil, and to prevent its being deposited in a solid form. I quite agree with Captain Selwyn. It was an omission on my part not to refer to the prejudicial effect of such a deposit of carbon upon the heating surface, in reducing its conducting power. It is no doubt from a cause like that—from the carbon encrusting the heating surface that cases of this sort have arisen: —a steamer would make her trial trip burning only 4 lb. of coal, per indicated horse-power, per hour. She would start upon her voyage burning the same quantity. The consumption would gradually rise to 4½ lb., 5 lb., and so on, and at the end of the trip she would be burning 6 lb. of coal, per indicated horse-power, per hour, without any deterioration in the working or in the management of her engines, but solely from the loss of conducting power, owing to the deposition of the carbon upon the surface of the tubes.

Captain SELWYN: May I say that I particularly wish to have an opinion from you with regard to the difference between the action, chemically and mechanically, of the blast in the chimney and the blast in front of the furnace?

Professor RANKINE: I think there is one difference with respect to the mechanical action—that a blast directly into the furnace is much more efficient. It produces a more thorough mixture of the inflammable gases with air than the chimney blast does. A chimney blast in a locomotive is a very convenient thing; but I do believe that you can produce an equally thorough mixture, or a more thorough mixture by direct blast into the furnace with less expenditure of air. Then, as to the indirect chemical process to which reference has already been made. Of course, if you blow steam into the chimney it does not take place at all. That chemical process that tends in an indirect way to economy of fuel can only take place if you introduce steam in contact with the inflammable gases. I am glad that Captain Selwyn has referred to this matter, because I ought to say something as to the temperature at which steam is introduced. Captain Selwyn has referred to this fact, that it may be said a certain quantity of heat is wasted in generating this steam, but that heat is made available again. Now, that depends, I may say, almost entirely upon the temperature at which the steam is used. You expend a certain quantity of heat in evaporating water, and you send the steam in at a comparatively low temperature. In the ordinary state of saturated steam, the temperature of that, as compared with burning fuel, is cold. It may be very hot as compared with the ordinary temperature of the atmosphere; but as compared with the temperature of fuel in the furnace, it is a very low temperature indeed. A body cannot give out heat to a body that is hotter than itself, so that the heat spent in producing steam at a comparatively low temperature will be wasted; but if you use superheated steam, in the first place a much less quantity of steam will serve the purpose. The mechanical effect depends upon pressure and volume. In order to get a great pressure and a great volume with small weight of material you must employ a high temperature; you must, in short, use superheated steam. As a great part of the expenditure of heat in producing the steam depends on its weight, the use of superheated steam tends to lessen the expenditure of heat for a given mechanical effect. Then, if you raise the steam to a temperature approximating to that of the flame itself, you get back nearly the whole of the heat again; because the superheated steam, being at a higher temperature than the heating surface of the boiler, gives out a great part of its heat to that surface just as the flame does.

The CHAIRMAN: I am sure that the meeting will join with me in offering our thanks to Professor Rankine for this lecture—I may say for this lesson. As an old Naval Officer myself, I feel that during the time when I was captain of a line-of-battle ship I should have been very glad of such a lesson as we have had to-night. I may also add that I hope it will be of great use, not only to us who are going out of the profession, but to those who are rising up in it. I must also make another remark—that the small number who are here present is no measure of the value of a lecture such as this, or of the good that it will effect. There are many others who, thanks to our Journal, will have the opportunity of studying this lesson, Naval Officers especially. There is a great deal in the professional part of the question which Professor Rankine

has thought it right to leave out. But points have been touched upon very ably by Captain Selwyn in almost every one of which I cordially join, because I think those points are of great importance. The efficiency of our Navy will very much depend upon the fuel we shall use in the future, and upon the way in which we shall be able to economise the use of it, or economise the space for it in stowage. Every pound of fuel we shall be able to save hereafter will be so much space gained; not merely space gained for air and so on, but space for efficiency, for the life of the ship, because her efficiency depends upon the quantity of fuel she can stow. She is no man-of-war if she goes to sea devoid of fuel, and is met by an enemy with a good supply. There would be an end of her. We must also remember that all we can gain in the way of space and efficiency will enable us to carry a better armament, and to devote the space saved to other purposes, even if we do not devote it to the life of the ship, viz., the fuel she carries; for fuel to a ship is as the air we breathe. I must again ask you to join with me in thanking the Professor for the very able lecture which he has given us.

Evening Meeting.

Monday, June 3rd, 1867.

Major-General J. T. BOILEAU, R.E., F.R.S., in the Chair.

NAMES of MEMBERS who joined the Institution between the 20th May and the 3rd June, 1867.

ANNUAL.

Rochfort, Charles G., Capt. 20th Regt., £1.
Winthrop, Hay, E.S., Capt. R.N.

FURTHER PARTICULARS REGARDING MONCRIEFF'S PROTECTED BARBETTE SYSTEM.

By Captain A. Moncrieff, Edinburgh Artillery Militia.

Since I had the honour of first introducing this subject here on 3rd June, 1866, I have not made so much progress with my invention as I could have wished, but such progress as my own means have enabled me to make has been of a satisfactory kind, and it affords me much pleasure to have this opportunity of reporting the results.

As probably the best way of re-introducing the subject, I shall briefly sketch the history of the invention up to this time, defining it in as few words as I can.

The invention consists in mounting guns in such a manner that the act of firing makes the gun descend below the parapet, where it is secure from direct fire, and where it can be loaded and traversed by the gun detachment, without their being exposed. The force of the recoil, which drives the gun back and takes it down under cover at the same time, raises a counterpoise, and the spare energy of the recoil is stored in raising the counterpoise, and on the gun being released from the loading position, this spare energy brings it up into the firing position without labour. The counterpoise, moreover, is attached in such a way that the shock of the discharge is gradually conveyed to it (this

being a most important condition where such large weights have to be put in motion). There is also interposed between the gun and counterpoise, a moving fulcrum, which has the effect of taking off horizontal strain from the platform. In this, and the system of defence based on it (where all the guns have free sweep, and can be mounted in gun-pits or in batteries without a visible parapet, or below water level), consists my invention.

The idea first occurred to me in 1855. I was impressed with the advantages that would be gained by dispensing with embrasures, even with the old ordnance, while watching on the spot the operations before Sebastopol. This impression led me to try to invent some simple means of raising the guns for firing, and lowering them under cover for loading.

It is difficult now for me to describe the path by which I arrived at my present conclusions, but that path was paved with many plans for arriving at the result now so simply obtained. I brought into requisition every mechanical appliance I could think of, that appeared suitable, and a good many pretty designs were the result; but simplicity was not their general characteristic. At last I conceived the idea of interposing a moving fulcrum between the gun and a corresponding counterpoise, which is now the principal characteristic of my invention.

I submitted my first plan based on this principle, in 1858, to General Sir R. Dacres, under whose command I happened to be at the time, who thought it of sufficient importance to bring it under the notice of the Secretary of State for War.

It is probably better that I should not now go into all the hopes and disappointments I have had since in attempting to bring it forward, but I shall pass on to the time when I read my paper here last year.

I was much encouraged to persevere, by the reception I met with from this Institution, and shortly after reading my last paper, I submitted the invention once more to the Government—I am sorry to say, with no better success than formerly. I had now three courses left open for me—to let the matter drop—to take it out of England—or to conduct an experiment with a full-sized gun at my own expense, in order if possible to remove the scruples which Government had shewn to incur the expense of trying my plan. I chose the last course. General Peel was good enough to supply me with an iron 32-pounder, which I mounted in the neighbourhood of Edinburgh on my principle. In order to save expense, I was careful that the principle only should be thoroughly tried in my carriage, but did not attempt to make it a pattern for the Service; it was simply an experimental carriage, and its defects were such as to make successful action more unlikely, and therefore, at the same time, more conspicuous. Those members of this Institution who had faith in the soundness of my principle, when it had only been tried with a working model, will share my gratification at the results of this experiment. Not only did the carriage stop the recoil without strain, but it did so with a smoothness and an absence of vibration that at once satisfied those who saw it, that as a mere mechanical appliance it had good working qualities. But to those who looked a little farther and were aware of the destructive effects caused by vibration and the enormous horizontal strain on the platform in

firing modern heavy artillery, the experiment suggested the possibility that my system might not only answer for the largest class of ordnance, but might, in addition to its other advantages, remove the difficulty now felt of providing sufficient foundations for batteries and traversing platforms strong enough for the largest guns.

The following is the memorandum of experiment conducted at Edinburgh:—

Memorandum of Experiment.

Edinburgh, 3*rd December*, 1866.

"In 1865, a model to illustrate this invention was constructed. It represented a 95-cwt. gun on $\frac{1}{8}$th scale. This model was experimented upon in presence of Colonel Dickson, C.B., V.C., then commanding the Artillery in Scotland, and the officers of that arm of the service stationed at Leith.

The result, as far as could be obtained by the model, was most satisfatory; but it is well known that model experiments are not always conclusive, and to remove doubts as to the principle on which the invention is founded, the present experiment is about to be tried.

To avoid expense, the mounting of the gun to be now tried, is of the rudest description, compatible with a full and clear illustration of the principle; and it is evident that such rudeness is against the invention, and not in the inventor's favour.

The gun is a 32-pr., on its own carriage. The elevators are a pair of locomotive driving wheels, of 7 feet diameter, fitted up for the purpose, and mounted on two ordinary rails secured upon sleepers, to represent the platform. Two axles near the circumference of the wheels connect them together, from one of which the counterweight is suspended, and on the other rests a timber frame, on which the gun on its carriage is secured.

The gun, its carriage, and the frame, represent a weight of about 68 cwt., and the counterpoise is about 78 cwt., giving a preponderance to the weight of about 10 cwt.

The points to which the inventor would solicit special attention are as follows:"—

1st Shot.

Details.	Remarks.
Charge 8 lbs. powder and 32 lbs. shot.	Filled in by Col. Burgman, R.E.
1. Distance the wheels travelled on the rails.	3 feet 3 inches.
2. Recoil smooth and steady, or accompanied with vibration,	Smooth and steady.
3. Recoil gradually checked, and the gun brought to a stand without jar or jerk.	Quite so.
4. Whether or not any vibration that would injure slight masonry works, is transferred to the platform.	No visible sign of vibration in the platform.

2ND SHOT.

Details.	Remarks.
Charge, 8 lbs. powder and two 32 lbs. shot.	
1. Distance the wheels travelled.	5 feet 3 inches.
2. Recoil smooth and steady, or accompanied with vibration.	Recoil smooth and steady, without vibration.
3. Recoil gradually checked, and the gun brought to a stand without jar or or jerk, or the contrary.	Recoil gradually checked, but it was sufficient for the whole length of the rack to pass over the paul, and when the gun began to run up by the action of the counterpoise, the paul jumped twice before it caught. The rack and paul were rudely made, and were quite a temporary arrangement.
4. Whether or not any vibration that would injure slight masonry works is transferred to the platform.	None.

(Signed) G. H. BURGMAN
Com. R.E. in N.B.

"I witnessed the above experiments, and agree in the remarks made by Colonel Burgman, R.E., as to the results of each shot.

(Signed) "FRED. WM. HAMILTON,
"Maj.-Gen. Commanding.

(Signed) "J. R. ANDERSON,
"Col. Commanding the
"R.A. in N.B."

On the 28th November, 1866, I wrote to the Right Hon. the Secretary of State for War, informing him that I had completed my experimental carriage, and would be glad to know if he would appoint any one to report on its performance, or to subject it to any tests that might be required.

On the 12th December, I received an intimation that an Officer of the Ordnance Select Committee had been directed to proceed to Edinburgh to test my carriage. On the 14th December, Colonel Wray, C.B., of the Ordnance Select Committee, arrived.

I shall quote a portion of the report he made to the Ordnance Select Committee—that portion which applies to the actual experiments.

"The following rounds were fired:—

Round. No.	Charge. lbs.	Shot. lbs.	Recoil.	
1	8	32	4 ft. 1 in.	cap square of garrison carriage cracked.
2	Repeated		3 ft.	
3	13	0	4 ft.	4° elevation.
4	8	Double shot	5½	
5	Repeated		5	

"Tackle substituted for the rack in the last round, the gun recovered "its position without jerk or strain.

"Nothing could be more perfect than the working of the whole "machine under the above trials; the recoil was smooth, and the gun "being brought to a stand after each round without jar or jerk, and

"there were no visible signs of vibration in the platform or any part "of the structure.

"The rack and paul did their work perfectly, carrying out to the "letter the principle (as described by Captain Moncrieff) of storing "the energy of the recoil, which, on being released, quietly allowed "the gun to recover its fighting position through the agency of the "counterpoise."

The report then goes on to criticize the application of my system to large guns, and to point out the possible inconvenience of large counterweights and the difficulty of traversing, &c., &c.

The most satisfactory, and perhaps the only way of solving such questions is to put them to the test of trial; and it is only denied by a few, that the issue is of sufficient importance to justify a series of experiments. Those who are best acquainted with the present question of fortification are probably at the same time those who will put the highest value on an invention that will, with simplicity of action, perform the function which mine performs. I have great hopes that Government will still authorize a trial, notwithstanding their last refusal. The necessity for some plan of the kind is so generally allowed, and my design promises so many advantages, especially as applied to coast defence, that it appears wasteful on their part to continue the expensive works required for present appliances, while my system offers any probability of success.

I shall now take the liberty of reading only two letters, one from a civilian, and the other from a soldier. This meeting will understand how to value the testimony of both. I am sure that I could not select two authorities whom I should better prefer to rely on.

(1)

"*From W. MacQuorn Rankine, Esq., Professor of Civil Engineering, &c., &c., University of Glasgow.*

"To Henry Glassford Bell, Esq., &c., &c.

"59, St. Vincent-street, Glasgow,
"17th April, 1867.

"My Dear Sheriff,

"It would be beyond my province to give any opinion as to the military merits of Captain A. Moncrieff's method of mounting guns; but in a purely mechanical point of view, I can confidently state that I consider it both sound in principle, and likely to answer well in practice.

"I am, yours very truly,
"W. P. MacQuorn Rankine."

(2)

From Colonel Simmons, C.B., R.E., Director Royal Engineer Establishment, Chatham.

"Royal Engineer Establishment, Chatham,
"18th March, 1867.

"To Captain Moncrieff, &c., &c.

My Dear Sir,

"I have been looking lately with much interest at the description and plans of your proposed system for raising guns, so that they may fire over a parapet, and in their recoil fall down below it, so as to be completely concealed from view. The object, the solution of which you have proposed to yourself, is one of very great

importance to the service, and one which, before I knew you had turned your attention to it, had occupied mine so much, that I had tried to direct the attention of my brother officers to it, and I also suggested it as a problem requiring solution to some mechanical engineers, thinking it might probably be accomplished in a convenient manner by hydraulic power.

"The importance which I attach to an invention of this nature is very great. By it the gun is effectually concealed when not in action, and is kept under cover for the greater part of the time it is in action. The gun, when placed behind a parapet or epaulment, or in a pit, presents no object upon which an enemy can direct his fire; the importance of this, when opposed to rifled guns (both small and great), cannot be exaggerated, and moreover, it disposes of the difficulties attending the use of embrasures in earthworks, whether for attack or defence. These difficulties are very great:—

"1st. The embrasures present a fixed and constant target upon which guns may be laid.

"2nd. Embrasures weaken a parapet, and, as usually constructed, present the most favourable conditions for bursting shells fired with percussion fuzes, the thin part of the merlon affording just resistance enough to fire the fuze.

"3rd. The gun is always more or less exposed to injury from direct fire.

"4th. No revetment has yet been found for the cheeks of embrasures, which is not readily destroyed, either by the fire of their own guns or those of the enemy, thus shutting the guns up and necessitating repairs, which are among the most dangerous duties of the soldier.

"Various means have been proposed for palliating these evils, such as fixing iron shields, revolving cupolas or towers, and many other schemes which lessen the efficiency of batteries, by restricting their lateral range, and, all of which that I have ever seen, are exceedingly costly, and after all are only a very partial cure for the evils complained of.

"It appears to me that the system of loading guns below the parapet, which may be of earth of any thickness, and therefore very difficult to destroy, and only bringing them up to an exposed position at the moment of firing, gets rid of all these difficulties.

"Of course any system which may be proposed for this purpose must have objections of its own; but I confess that your scheme is more free from objection than any I have seen: and having given it my best attention, I see no reason why it should not succeed with guns of any weight, however great, that are ever likely to be introduced into the service. If successful, it will save an enormous outlay to the country in its fortifications, which, in these economical days, is almost more thought of by those who control expenditure, than efficiency,—at the same time that it will add enormously to their practical value when submitted to their true test by an enemy's fire.

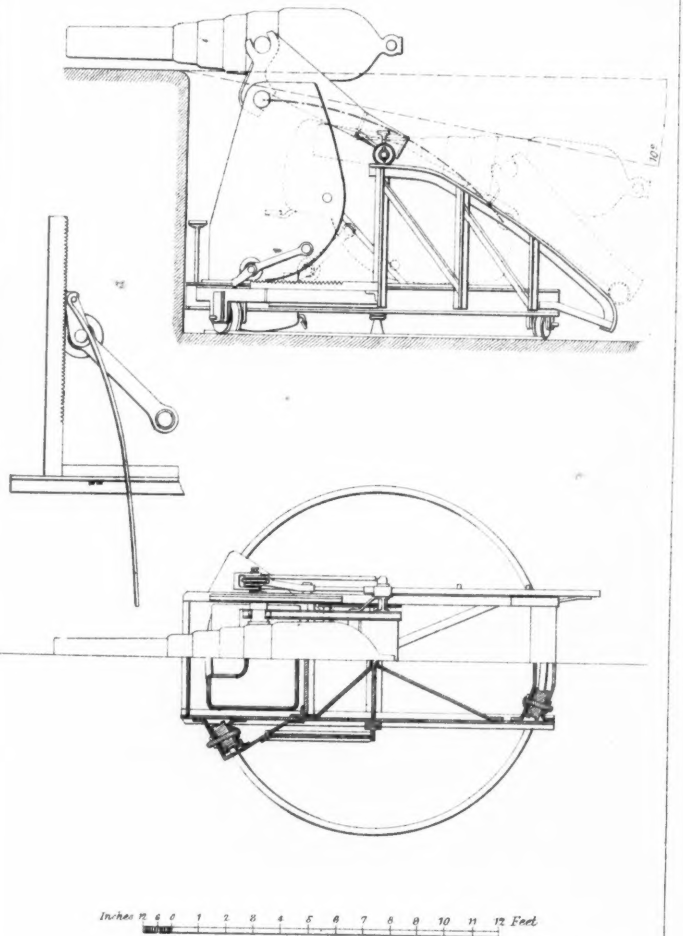
"Your scheme appears to me to present no mechanical difficulties but what might easily be overcome, and it gets rid of one great mechanical difficulty which has not yet, I believe, been solved with guns as now mounted on the most approved pattern of carriage and traversing platform. I allude to the horizontal strain brought by the recoil upon the various parts of the carriage, platforms, racers, and traversing bolts. This action is a very serious difficulty with guns on traversing platforms, whereas, according to your system, there will be little or no tendency to force the turn-table or traversing platform back.

"You may expect objections to be taken to the weight and bulkiness of your counterpoise; but I don't think they need disturb you, as, when once the gun is mounted, this counterpoise actually diminishes the labour of working the gun, forming, as it were, a reservoir of power to run it up; and with regard to its bulk, you may reduce that considerably by the employment of lead, cast in ingots, so as to pack very closely, and still be manageable on the rare occasions when it may be necessary to dismount the guns.

"I see no difficulty whatever in constructing a proper turn-table, for, after all, the weights to be dealt with are not greater than are to be seen daily on turn-tables on railways, and absolutely nothing compared to what may be seen in operation on board ship with cupolas and revolving towers.

Sketch of Proposed Adaptation of Moncrieff's System (without Details), to a 7 ton Gun.

May, 1867.



J. Jobbins

"One great objection which may be raised to any system of this nature is, that it is not compatible with the protection of the guns from vertical fire. The question between guns protected in this way and others in casemates is therefore one of the relative danger of horizontal fire at embrasures and vertical fire. For my own part, I should not hesitate to choose in favour of the system as worked out by you, the gun being protected on its flanks and rear by traverses, which would reduce the danger to a minimum.

"There are many situations, however, where casemates are inapplicable, and, when opposed to shipping there is very little danger to be apprehended from vertical fire. In such situations I have no hesitation in saying, that some plan such as this of yours, if it succeed, will be an immense improvement on anything we now have, and will be invaluable. I hope, therefore, you will persist with your system, and get it thoroughly tried.

"Believe me, dear Sir,
"Yours very truly,
"J. L. A. SIMMONS."

Having so far explained the position of my invention, I shall now glance at a few of the adaptations of it that have occupied my attention.

I have endeavoured to apply my principle to an ordinary 40-pr. Armstrong siege-gun, taking its carriage as it now stands, and turning it into a temporary elevator on my principle by the addition of a light traversing platform, two plumer blocks, and two wheel blocks, as shewn in the drawing before you.

I have also applied it to an ordinary 95-cwt. gun in several ways; drawings of some of these are on the table.

I have also applied it to a 7-ton gun, using for elevators two $\frac{9}{16}$th plates, collapsing at the axle on forged blocks of iron, and strengthened and held together by] iron, securely rivetted to both plates (see plate xv).

The carriage in this case is supported at the rear on two curved rails, which guide it in its descent from the firing to the loading position.

The frame traverses on a racer 12 feet 4 inches in diameter and central pivot.

I have also planned a smaller carriage, where the trunnions themselves play in the elevators, without the intervention of the gun-carriage proper.

Having suggested a few of those forms in which my principle is likely to be applied to ordnance, it is perhaps better not to anticipate the results of next experiments, which will probably be tried by Government.

My own experiments, the reports of which have been laid before you, are sufficient to decide, at any rate, the soundness of the principle, and justify some confidence in the success of what remains to be done, if all details are properly attended to, in the manufacture of the carriages. I shall, therefore, leave that part of the subject, which deals with the carriage itself, and direct your attention to the advantages, at once within reach, when a complete carriage, made on my system, shall be obtained.

I shall classify the characteristics of my system under a few heads,

under each of which a comparison can be made with other systems, and the balance struck.

1st. Economy of labour in working the gun.
2nd. The power of masking the battery.
3rd. Power of meeting recoil, and removing horizontal strain.
4th. Power of traversing.
5th. Economy obtained in the construction of works independent of the carriage itself.
6th. Comparative protection from vertical fire.
7th. Comparative protection from direct fire.

1st. Economy of Labour in working the Gun.

In regard to the first point, viz.:—"Economy of labour," it is well known how much the demand for greater power in our artillery, has increased the weight of the guns. A few years ago a 95-cwt. gun was pointed at as a formidable engine of war, but now, a 7-ton gun is the smallest that is considered worthy of any consideration as a garrison gun—and 7, 9, and 12-ton guns are looked to as the ordinary guns for coast defence—not to speak of the problematical 23-ton gun. Even those who are not acquainted with working guns, can easily imagine the difficulty of dealing with a mass of iron 9 or 12 tons in weight, and the number of men that are required to do so efficiently. To cure this evil, I apply the homœopathic maxim, "*similia similibus curantur*," though not, I confess in homœopathic doses! I employ one elephant to tame another;—to overcome the objection to one great weight, I employ another, and the result is, that the gunner has only to deal with the difference between the two; instead of having to move 12 tons, he has only to regulate the application of energy exerted by the recoil, and the gun, as it were, almost works itself. Thus, the greatest application of power required from the gun detachment is to traverse, and that, as I shall explain under head 3, can be reduced.

The labour of lifting the projectile to the muzzle can also be dispensed with, as my carriage lifts up its own shot to a convenient position for loading; those who have gone through this straining operation, with large projectiles, will appreciate the advantage of having it done so easily.

2nd. The power of masking the Battery.

The power of concealing the position of batteries before action, gives an advantage that has made the practice of masking batteries, one that is commonly resorted to. A battery on my system can enjoy this advantage to a greater degree than one in which the guns are in embrasures, or mounted *en barbette*, or in a cupola; because the parapet may be *made* to resemble, or may actually be the natural surface of the ground itself, with nothing on which the eye can rest, nor at which the enemy can aim.

Of course, for the few seconds during which the gun is laid, it

must be exposed in the same manner as the guns are in *barbette* batteries, but that exposure ceases at the moment of discharge.

I shall here explain more fully than I did in my last paper, an appliance which would probably be invariably used in batteries made on my system. This appliance is a transparent screen, capable of being raised in front of each gun, or removed at pleasure. There are many convenient materials of which this screen could be made, which would vary according to the ground and the circumstances; and if no other material was at hand, small brushwood would do. The effect of using such a screen would be, to give an important advantage to my battery over those opposed to it. As the enemy would no longer be able to see whether my gun was up or down, and the first intimation he would receive of the gun being up would be its fire, after which to run the chance of hitting it when up, he must continue to fire at it all the time it is down; and as the gun when down is absolutely secure from direct fire, the enemy would not only have to overcome the chance of missing the position of the gun, but in addition to that, he must waste his fire in the same proportion that the time during which my gun is down bears to the time it is being laid, which proportion might be increased or decreased by those in my battery at pleasure, by slackening or increasing the fire.

3rd. Power of meeting Recoil and removing Horizontal Strain.

The recoil of guns up to this time has been stopped by applying friction in one form or another, and since the introduction of the new and powerful artillery of the present time, it has been found necessary to increase that friction. The most approved method of so doing is by American compressors, by which almost any strain can be resisted.

This convenient appliance, however, involves the necessity for great strength in the platforms, which, through friction, are made to resist the force of the recoil.

The destructive effects of this force and the vibration that accompanies it, was tested some years ago, I believe, at Portsmouth, when the firing of a large gun shook to its foundations the work on which it was mounted. My experiments at Edinburgh gave satisfactory evidence of the power of my carriage to destroy that vibration, and it is easily seen how the interposition of a moving fulcrum between the gun and platform, removes or modifies the horizontal strain.

In elucidation of this, I shall quote the following from the memorandum which accompanied Colonel Wray's report to the Select Committee on my experiments:—

" The application of this invention to the heaviest ordnance I con-
" ceive to be the most important of any, as it is calculated to give pro-
" tection and lateral range to the most valuable, and the most powerful
" ordnance. In approaching this question, the two difficulties which
" naturally arrest the attention, and at the same time the only ones
" which would interfere with the adoption of the system, are—

" 1st. The difficulty of dealing with a large counterweight; and 2ndly,

"the difficulty of traversing. With regard to the first, I should wish "to direct attention to the fact that the impact of the recoil is reduced "to a minimum on the counterweight, from the initial velocity of that "counterweight being so small. The movement of the counterweight "increases gradually, representing an animal lift, and the direction of "all the forces brought into play are changed on curves, causing a "destruction of vibration exceedingly conducive to smoothness of "action. These conditions are exactly what are wanted for moving "large weights, and the cubic contents of the counterweight in iron "is not so great as to interfere with the gun being efficiently served."

"With regard to the second difficulty, it will be observed that this "carriage differs completely from others in this respect—that the "horizontal force of the recoil on the platform is removed in the "same manner that it is removed from a common railway when one "locomotive pushes another before it. This being the case, the "strength that is necessary to support the weight in repose is nearly "enough; and as there is no need to provide for horizontal strain, "the difficulty and expense of providing a traversing apparatus with- "out friction, is in a great measure removed."

4th. The power of Traversing.

The traversing apparatus which I have designed for a 7-ton gun is of a very simple nature. It consists of a single complete circular racer, 12 feet 4 inches in diameter, and a central pivot; and in all the other applications as well, the amount of lateral sweep is restricted by no condition. This is probably the most valuable advantage of my system, in conjunction with the protection afforded while the gun is down. It is the free lateral range that makes it so valuable for coast defence, where guns in many positions may be said to be almost useless unless mounted *en barbette*, and it might be added that they would soon be rendered useless also, if mounted *en barbette*, when exposed to an enemy's fire.

For salients and detached works, or where used in my proposed gun-pits, no other system except the cupola can command the same range with the same protection.

5th. Economy in the construction of Works, independent of the Carriage itself.

This is perhaps the most important consideration of any in connection with my invention, and the one on which I would prefer to base my claims to attention from the Government. But as I cannot enter into it with the detail which it deserves, I shall take the liberty of passing it over altogether—merely referring to Colonel Simmons' remarks on the subject already read, and also to the quotation from the professional papers of the Royal Engineers in my last paper regarding the cupola—and suggesting that the more closely this point is investigated, the more enormous will appear the extravagance of the present system, compared with mine.

6th. Comparative protection from Vertical Fire.

As before stated, my design makes a heavy gun work on a circular racer only 12 feet 6 inches in diameter. The central pivot might be placed in the line of the inner side of parapet; and the front part would then occupy a semicircular recess, the rear part being protected in flank by an ordinary traverse only 8 feet in length. This would limit the exposure to vertical fire to a very small superficies; but it would probably be better to have 16 feet traverses and a square front, as the angles between the traverse and the parapet would be the safest, and at the same time the most convenient place for the men working the gun, and it would still bear favourable comparison.

7th. Comparative protection from Direct Fire.

The protection which my system affords is of a character that has not as yet been given to artillery. In working guns, two conditions which usually conflict with one another have to be obtained; the one is to make the gun formidable to an enemy, and the other is to have at the same time both it, and the men working it, as little liable to injury as possible.

The first condition is obtained by having appliances that expedite aiming and loading, and also those which enable each gun to traverse as large an angle as may be required. The second condition has until lately received very few improvements beyond the old and well-known method of the embrasure, &c. However, improved casemates, the contraction of ports by the use of armour-plating, cupolas, and Lieutenant Bucknill's ingenious system of firing through a false parapet or screen, &c., &c., have each and all their advantages for certain positions, but with one exception, viz., the cupola, they all curtail the power of the gun by contracting its free range; and, therefore, with that exception, what they gain in safety they lose in efficiency, where range is required. My system has the happy peculiarity of combining these two conflicting elements in a high degree.

The embrasure necessitates the breaking and weakening of the parapet. It also restricts its thickness for a given number of guns, not to speak of the mark which these embrasures present to the enemy. Armour-plating on land works, at great expenditure of money, reduces these evils considerably, but by no means entirely removes them; so that on reviewing the position of my invention in this respect, I feel my only competitor to be, the cupola. What can be said in favour of the cupola nearly applies to my system. We are equal in our power of traversing; and in the matter of protection, you will have to decide, after I have stated the exposure in each case.

The cupola is always a mark, and is always exposed, and to very heavy ordnance, its invulnerability is still problematical. Its port,

though small, is liable to be hit for a certain time. Its gun detachments are annoyed, if not hurt, by the concussion of heavy projectiles. It is of enormous weight, and to avoid a shot in the port, it requires to turn its cheek on the enemy after each round, involving a good deal of labour. On the other hand, my gun, and the men serving it, are absolutely protected from direct fire, except that the gun and one man are exposed while aim is being taken.

When the gun is up to be aimed, it is more exposed than the cupola gun, but the moment it is fired, it is safe.

If a screen be used, the enemy cannot see whether the gun is up or down; I thus draw his fire, the correctness of which must be materially affected by having no definite object to aim at.

The best way perhaps of putting the question is this; would gunners prefer to be shut up in an iron box, only penetrable by the enemy's shot through the port, but liable to injury, in other parts; or would they prefer to fight their guns in the open air, and all under cover, except the man who aims, he being only exposed for a very short time and partially protected by the massive breech of the gun? Are the chances of injury greater in my case, where the gun is only liable to be hit during the few seconds required to lay it, and is in absolute safety the moment it is discharged—or, as in the other case, where the cupola remains a constant mark for heavy projectiles, and runs a continual chance (though a small one) of receiving a shot through the port itself?

I do not care which way this question is answered, as I have other and independent arguments in my favour.

With the exception of field guns, no artillery is brought into action without the artificial protection of a parapet, which is generally raised above the level of the surrounding country, and pierced with embrasures. This parapet is the mark at which the enemy directs his fire, in order that by destroying it, the guns which it screens, may be disabled.

It will at once be obvious that the possibility of dispensing with a parapet without losing command of the front of the battery, would give an advantage of an important kind. This advantage I seek to obtain in its greatest degree by employing gun-pits, in which all the vital parts of the carriage remain below the level of the surface, and the gun itself is only exposed when it is going to be fired.

For coast batteries liable to be opposed to the heaviest artillery in ships, a very strong work is now absolutely required to protect the guns from the terribly destructive effects of modern projectiles, which have a penetration far beyond what was dreamed of when most of the existing fortresses were built; and as accuracy of fire has increased, as well as its power, the guns cannot be mounted *en barbette*.

In order therefore to be efficient, coast batteries must be of great strength, and proportionately expensive, especially when iron is used in their construction.

I wish this to be borne in mind, while I point out that by taking advantage of the natural undulations of the ground, scarping down the rear of hillocks to make them into batteries, and applying the skill of

our military engineers to use whatever nature has supplied in each place, many positions might be defended on my system from the attacks of the heaviest artillery, at a small percentage of the cost which is now required to construct batteries with iron embrasures, cupolas, &c.; and that notwithstanding the economy of these works, they would be probably as invulnerable as their more expensive rivals.

I have dwelt longer than I should have done, upon the comparative advantages to be gained by my system, as I know that some Officers who are aware of the working properties of my carriage maintain that the object to be gained, is not of sufficient importance to justify any change.

It is true that the change will make a revolution in the system of working heavy artillery, and that change will probably mark an epoch in fortification; but if the change is forced on us by the increased power and accuracy of guns, it is useless to attempt to rest on what is past, and it is best to take the most efficient and economical plan for the future.

In my last paper I stated my strong conviction that my principle must some day be adopted, and I gave as my reason for thinking so, that it is calculated to effect an enormous economy of *labour*, *material*, and *life*. I have for eight years endeavoured to bring my system forward without success, but my failures in doing so, cannot convince me that I am wrong, nor will they make me desist; from my feelings being old fashioned in such matters, I have all along declined to ask for that satisfaction, which I know I could get abroad, before first receiving it in my own country—and I console myself with the reflection that the time will come, when it will only be a matter of surprise that so simple an application, and one so consistent with common sense, was not sooner adopted.

I take this public opportunity of expressing my gratitude, and thanks, to the following Officers, for the encouragement and support I have received from their good opinions of my invention; and for the readiness with which they have at one time or another aided me:—

Lieutenant-General Sir R. J. Dacres, K.C.B., R.A.

Major-General Collingwood Dickson, V.C., C.B., R.A.

Colonel Alaric Robertson, Indian Staff Corps.

Colonel Shafto Adair, F.R.S., A.D.C. to the Queen.

Captain Schaw, R.E., Professor of Artillery and Fortification, Royal Staff College.

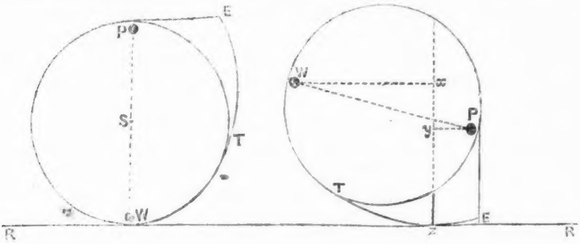
Colonel Jervois, C.B., R.E., Deputy Director of Works for Fortification.

Colonel Simmons, C.B., R.E., Director Royal Engineer Establishment, Chatham.

I feel more particularly indebted to Colonel Simmons for his open support, as his experience in actual war, and his high scientific name as a soldier, not only make his testimony gratifying to myself—but convincing to those who do not care to look into the subject for themselves.

The CHAIRMAN: Perhaps you will kindly explain the model on the table, Captain Moncrieff?

Captain MONCRIEFF: The model before you is not a very perfect one, because it was made at an early stage of the invention, and does not shew it in its best form with recent modifications and improvements, but it is quite sufficient to illustrate the principle. The gun is now in the firing position, and of course the discharge has the effect of running it back. I can, however, better explain the action by means of a suitable diagram or two. Let P represent the position of the gun-carriage axle, and W the centre of gravity of the counterweight; the moving fulcrum always being the point of contact between the wheel or elevator and the rail R R. It follows that while the wheel rests on any part of the circumference W T, the points W and P will be equally distant from the perpendicular to the rail R R at the point of contact between that rail and the wheel: but when the recoil moves the elevator back until the point of contact with the rail is the point z you will



observe that the weight-arm of the lever W x is increased, and the power-arm P y is diminished, and every further inch of recoil lengthens the weight-arm, and shortens the power-arm, until the latter is reduced to nothing; that, however, could not occur, because no discharge could have power to do it. The resistance goes on in an increasing progression, and must inevitably but gradually stop the recoil. I hope I have made that distinct. The force of the recoil sends the gun down, and when down, it is held by a paul. When the gun is to be brought into action the paul is lifted, and of its own accord the gun rises quietly.

Colonel GAMBIER, R.A., Adjutant-General of Artillery: In the case of a heavy gun, how do you load it? I suppose you load it when it is down.

Captain MONCRIEFF: You will see two steps, one on the frame and another attached to the counterweight. The gunners Nos. 2 and 3 stand facing one another upon these steps, in which position they can load.

Colonel GAMBIER: In the case of a 32-pounder?

Captain MONCRIEFF: The case is precisely the same with a 95-cwt. or 7-ton gun. And you may observe that I have brought in here [pointing to another diagram], on the front part of the counter-weight, the appliance I alluded to in my paper for lifting the projectile. When down you place the projectile on it, and the force of the recoil lifts it up to the muzzle into the position in which it will be most convenient for the gunner, and that position can be rectified by the elevating screw.

Colonel GAMBIER: Looking at the model, it would seem to me difficult to bring the shot up to the muzzle of the gun. I mean in the case of a 600-lb. shot.

Captain MONCRIEFF: This is a model of a 95-cwt. gun, and this gun does not bring the shot quite up to the muzzle, but, you see, it brings it very nearly up; but as you may see by my drawings, any projectile, however heavy, can be raised to the desired position.

The CHAIRMAN: Perhaps Captain Moncrieff will be kind enough to take a note of all questions as they are asked, and favour us with his replies to them at the end of the discussion. The subject is one of very great importance, and I trust the

meeting will not break up without an expression of opinion on the part of some gentlemen present, who are doubtless quite competent to offer observations on the subject, which will interest the meeting. We shall be glad if any one will break the ice, and favour us with their remarks on some of the various points which the paper has put before us.

Commander COLOMB, R.N.: If I get up to make any remarks, it is only to carry out the wish of the Chairman, and break the ice, because it is not properly a naval Officer's business to open a discussion on a subject of this kind. When Captain Moncrieff read his first paper in this Institution, I was very much struck with the whole arrangement, as a wonderful and ingenious application of very simple mechanical appliances to supply what I had always understood previously, to be a great want. I then said that I thought, before we could express any decided opinion on the subject, we should like to have an experiment upon somewhat of a large scale. I think to-night Captain Moncrieff has given us ample grounds for expressing a very decided opinion, and I believe that opinion can only be of a most favourable character. He gives us not only exact details of his experiments, but he gives them not from his own point of view, but from that of independent, qualified witnesses. I look upon that as a very important matter, always to be attended to; because, however ready an inventor may be to disabuse his mind of prejudice, he can hardly fail to give a prejudiced account of his own works. To-night we have not had his own account, but the accounts, as I have said, of Officers qualified to deal with it, and yet perfectly unprejudiced. We, not having seen those experiments, can hardly fail to do right in most fully endorsing the report which he has read to us of the opinions of those Officers. I was surprised to hear Captain Moncrieff say that he understood that there was an opinion afloat that, even supposing guns could be worked in the manner suggested by him, yet there was not a field open for using them in that manner, because the present system of fortification was amply sufficient. Of course I do not attempt to express any distinct opinion upon that subject, but where the cost of fortification is so enormous, and where you have spent enormous sums of money upon different fortifications, and you are still not satisfied that you have done anything commensurate with the requirements of the case, I hardly think we can exaggerate the importance of any system of fortification which will reduce the cost. I do not think we need go any further than to express a disagreement with the gentlemen who expressed such an opinion. There is a question which perhaps Captain Moncrieff will take a note of—I do not put it myself, but it was put to me this morning—viz., as to the difficulty of firing, from the position of the man who has to fire the gun. I think it is quite a matter of detail, and has very little to do with the system, but still it may satisfy some to hear Captain Moncrieff's explanation. For my own part, I expressed a strong opinion when I heard the first paper read, and I can express a still stronger opinion now, viz., I think Captain Moncrieff's invention will lead to a great revolution in the method of working guns.

Captain JASPER SELWYN, R.N.: I should like to make one or two remarks. I should premise that I, like Captain Colomb, formed a very strong *primâ facie* opinion when the first paper was read in favour of the ingenious invention which Captain Moncrieff has brought before us this evening, in its altered form. I firmly believe, with Captain Colomb, that there is not the smallest ground for saying that there is no field for the application of this system. No doubt there is an increasingly large field for such gunnery improvements. Steam has produced such a certainty of locomotion and action, that it is certain that if a war did break out, it would be precisely those points which have not been amply fortified that would be the ones chosen by an enemy for attack, and it is only by being prepared, rapidly and efficiently to throw up temporary defences, carrying even the very heaviest guns, that we can possibly meet the changed conditions of defence and attack. I have no doubt that everything that goes on before us, or anywhere on our coast, is perfectly well known to those whose interest it may be one day—may that day be far distant—to attack us, and I say that the very last place which, as a Naval Officer, I should assail, would be a thoroughly well-plated fort. I should say first of all, that there was very little gained when I had taken it; and, secondly, that there being a thousand other points

of disembarkation, I should not trouble myself about it at all. I have come some distance to-day in order to have the pleasure of being present at the reading of the second paper of Captain Moncrieff: I have so great, so strong an opinion, that it is one of those inventions which is not only calculated to save millions of treasure to any nation that may adopt it, but also, what is still more valuable, human life. There is a point upon which I did not quite understand Captain Moncrieff, and which he will perhaps take a note of, viz., as to the distance at which he proposes to place his brushwood screen. It is clear that the firing of very heavy guns, or the enemy's fire in return, would very seriously interfere with an abattis, or anything of that kind, and it will be only something of the lightest character, or something that is placed at a considerable distance from the friendly fire, the fire of the gun itself, that would remain. The accuracy of the present artillery fire has reduced it to a matter of certainty, that guns without protection, would be very speedily disabled. This is known from some of the attacks on the Russian forts during the last war, where some of the largest guns were speedily silenced by Captain Coles' guns on rafts. I am not one of the advocates of Captain Coles' system, though I recognise its adaptability in certain cases; but I say as regards the difference between the proposal of Captain Moncrieff and cupolas, even leaving on one side the question of expense and difficulty of transport, I should have no hesitation in deciding in favour of Captain Moncrieff's system. There is another fact which I cannot forget. I have been down at Shoeburyness; I have seen targets fired at there, and I am perfectly certain that any chance shot at any moment may entirely stop the working of any cupola whatsoever, and that as for saying because we have fired at a cupola, or a cupola has been fired at and has not been stopped, therefore it may not be stopped, is to rely far too much on the chapter of accidents. The very slightest burr of the iron-clad deck, the very slightest bulge of the armoured cupola, will at once put a stop to all that smoothness of rotation which is claimed for these forts on pivots. I hope that Captain Moncrieff will be able to tell us also something more than I have yet heard of the possibility of the application of this system to ordinary gun-wheels, so that by substitution of gun-wheels, locomotion may be gained, now that it is so much required, and with regard to the substitution of the weight of the carriage itself, or the shot for the counterweights. I think, if I understand his diagrams aright, he has made some steps already in that direction. I quite clearly understood that the means of raising the shot—very heavy shot, 300 lbs. and 600 lbs., if you please—are much greater than have ever been obtained hitherto—that in all cases, whether it be that the man has to be raised as the gun goes out, or whether the shot has to be raised to the muzzle as the gun comes in, there is ample means of doing that by the very simple mechanical process which is here employed. We must recollect that the increased penetration of shot, as Captain Moncrieff has observed, gives us much thicker embrasures, that the thicker embrasures limit our fire, and that, however thick we may make them, the guns are still very ill-protected in the embrasures. In the case of exposed sea-beaches, we get a power by means of this system, which is most valuable. Gentlemen will recollect, that on a sea-beach there is generally a natural bank thrown up by the waves, and behind that natural bank, all that Captain Moncrieff proposes, can be established. We may send down an army of navvies to any exposed point of the coast, and in 48 hours there may be a battery of formidable guns erected, not known of before, of which the enemy is entirely ignorant, and is, therefore, not prepared to meet. He will be very unwise who attempts, by arguments derived from what I may call, vested interests, to say, "Let us wait till we are attacked, and find out the evils that we have to suffer, before we begin to prepare for them." The Army, too, we must recollect, by means of this system, can fortify themselves, without all the appliance of an engineer's force, formerly necessary. It may, as was done in America, carry out its fortifications on the spot; it may put its guns and riflemen under cover. The protection to the men loading, which is greater, or seems to be greater, in the cupola, is to be taken in connection with this fact, that the gun-pit itself is exactly like a cupola, with the difference of the thickness of the protection which being derived from the earth, is practically immeasurable, and, what is more, costs almost nothing. The protection to the men loading here is absolutely perfect. The protection to the men loading in the cupola, or any system in which

embrasures are used, is imperfect, and, if it is attempted to be made more perfect by the use of shutters, as in the American war, then the shutter itself is liable to be jambed, and the gun is disabled from that cause. Here there is nothing of the kind. I see, and every Engineer who looks at the plan which Captain Moncrieff has shewn us, must see, that his carriage and elevator are capable of enormous diminution of weight, that the parts can be made of mild steel—steel, which is to be regarded as good wrought-iron; that they can be lightened in every direction, that the strain thrown upon the gun is, as he describes it, one thoroughly taken up by the slow resistance of the weight. It is in fact an utilization in the very best and most important way, of the power which has been hitherto not only thrown away, but has required the force of a large number of men to countervail; the force of the recoil having, in all cases, to be met by the power of a number of men exerted to run the gun out again. There is another advantage which, I think, Captain Moncrieff did not dwell upon, and to which I should be disposed to attach enormous importance, that is, that this system gives us the power of fighting with fewer men. The great drawback to this nation always must be, the small number of men comparatively which it is able to bring into the field. It has wider possessions than, probably, any other equal number of human beings. These it has to protect against attack. I have often lamented, when abroad, the very small number of men that are available for artillery, or useful for military purposes in our distant possessions; but you are obliged to work with what you can get, and he who, as an Engineer economises and makes one man do what ten have hitherto done, has accomplished as much in his way as he who makes a blade of grass grow where one never grew before. I congratulate Captain Moncrieff on the forward steps he has taken, and I express my belief that no possible weight of gun can ever prevent this principle from having its fullest and freest action.

Captain MONCRIEFF: With reference to what Captain Colomb said about those who did not see the necessity for my system, I do not know that it is necessary for me to say much. Probably he may meet some of those gentlemen himself to whom I referred, and have an opportunity of answering their argument much better than I can do. Touching his question about the difficulty of firing the gun, I have heard that difficulty started before, but I believe that it is a matter of such small detail that it is not worthy of being raised as an objection to a system which has so many other advantages. It has been urged that the lanyard could not be pulled at right angles to the friction tube. I can only suggest that if that circumstance interferes with the certainty of firing, it can at once be remedied by placing a small shoulder-piece on the gun, over which the lanyard can be pulled at right angles to the friction tube. Captain Selwyn asked for a more complete explanation of the proposed plan of using a screen. I think that that would be also one of those details that could be carried out on the spot. What I now put forward in this respect is merely a suggestion. Sportsmen well know that they can see game perfectly through a bush when they themselves are concealed from its view. A small screen of brushwood some little way in front of the gun, made in parts, and to represent growing grass, and stretching along in front of the battery, but each part of which could be raised or let fall at pleasure, like the cover of this book, and pulled by a string within the battery, would be sufficient. With regard to the question about using the wheels of the gun-carriage as elevators, I think that my system is very well adapted for that purpose. In the event of our coast defence requiring it, it would be an admirable plan to have the heaviest guns furnished with such wheels. I have not got a drawing here in which such an application is shewn, but I have one which I made for Colonel Lennox, Instructor of Field Works at Chatham, who was impressed with the importance of my invention in siege operations, and he asked me if I could modify my system for siege guns. This is a sketch of the gun I sent him for that purpose, viz., a 40-pounder Armstrong, which is adapted without altering the carriage at all, but merely by adding a few parts. The design of this adaptation, I may say, requires a very slight exercise of the imagination to make it represent such a carriage as Captain Selwyn suggests.

Captain SELWYN: Perhaps you will put it on the wall and kindly explain it.

Captain MONCRIEFF: The parts coloured black represent the gun in a firing position. I may mention that the wheel shown is the wheel of an ordinary siege carriage. I

take the axle out of the centre of the wheel and mount it upon a block fixed upon the top of the wheel. There is an iron box or basket, which is filled with some material to make up the weight of the gun carriage, and the whole is mounted upon a small traversing platform capable of being put upon a waggon. The traversing platform is very light, and lies on the ground like two or three railway sleepers. The only extras that will be required to convert a common siege-gun carriage, besides the traversing frame, are the box for the counterweight, the blocks to be bolted upon the wheel for the carriage axle, and the curved blocks to be bolted on the tire. With these additions my siege carriage is capable of working, and any siege gun, by having these additions in store, might be converted at any time. All the guns of a siege train need not be made into elevators, but it would be a great advantage to be able to make them so if they were required.

The CHAIRMAN: What supports the trail of the gun in the firing position?

Captain MONCRIEFF: The block attached to the frame; the recoil takes place over that part.

Captain SELWYN: I understand Captain Moncrieff to say that the guns can be moved with the same facility as any ordinary siege train. Is the converse true that the guns could be mounted upon their wheels and used in the ordinary manner?

Captain MONCRIEFF: Yes, and brought into action either way. You made some remarks on the capability of diminishing the weight. My experience has taught me that it is my best policy to confine the experiments as much as possible to such simple forms as those you have seen, and that I must avoid refinements until my principle is taken up, as those refinements are always used against me; but at the same time I must add that I have improvements already designed, and that it will be my greatest pleasure to turn my attention to such matters of detail as may appear desirable by and by.

Mr. W. STIRLING LACON: I fear that I am out of order in asking a question which ought to have been asked before; but perhaps you will point out to us in a few words how you propose to lay guns in that position?

Captain MONCRIEFF: The man who lays the gun stands on a shelf between the cheeks, and at the rear of the carriage, and lays it in the usual way. He has access to this shelf, either by getting on it while the gun is down, and being raised with it, or else he ascends to his place by means of steps for that purpose on the rail, on which the carriage rolls back. When the gun is laid by my reflecting sight, the gunner who lays it stands under and in front of the gun, with his back to the parapet, and in that position he is able to elevate or depress by means of a handle such as those that are used for railway breaks. This handle works the elevating screw. The sight next him has a tangent scale, and enables him to aim with perfect accuracy without himself being exposed.

Captain BURGESS: How would the gun be traversed?

Captain MONCRIEFF: The gun is traversed in the same manner as is the present dwarf traversing platform; if the force required to traverse is inconveniently great, an ordinary purchase might be added to the rear trucks.

The CHAIRMAN: Before the meeting separates perhaps I may venture upon a few observations myself, although the subject has really been almost exhausted by those who have already spoken. There is one mechanical point, however, which has not been mentioned, and which may be important. It is this—that the action of these elevators is so arranged that at the time the gun commences to rise, the motion is less rapid, and when it approaches culmination at the highest point, the converse of that takes place, so that when in the greatest danger, it moves with the greatest rapidity, and the same may be observed in reference to its descent: it moves most rapidly while exposed, and is brought to a state of rest so gradually as entirely to do away with all jerk, and consequently with vibration. That I think is one of the excellencies of the mechanical arrangement; but it is not, to my apprehension, merely a question of mechanical arrangement—it is the principle on which the lecturer has chiefly dwelt, and it is my conviction that it must force itself upon attention. The mechanical details are matters of secondary importance, provided the principle is accepted. For instance, the counterpoise in this model is of a large unwieldy form, and not of a character to give a fair idea of what is likely to be

adopted. The weights may be brought into a very much smaller compass than they are; they may be made both smaller, and of a material of a much higher specific gravity; but that is a mechanical question. I may state, from what has transpired in this theatre, and the various interesting papers which have been read on the construction of heavy guns, that with regard to construction, we are still in a transition state. Guns which were assumed to be required, of seven tons or more, for throwing shot of a certain dimension and weight, are, I believe, now made very much lighter by the introduction of coiled iron, and steel cores into guns already cast, so that, if I am not mistaken in what I have heard here, and the conclusions which have been drawn therefrom, a gun of 95 or 100 cwt. will perform the same functions as one of 140 cwt. of the Armstrong or wrought-iron pattern. Should this be the case, the action of the mechanism in Captain Moncrieff's elevators will be relieved of one third of the weight on the gun, and the same on the counterpoise. But it is not as regards the arrangement of the mechanism or the weight, or construction of the gun itself that the question at present derives its greatest importance; it is as an engineering matter. The construction of gun-pit batteries at an expense altogether insignificant compared with others, whether cupolas, or protected by iron or in any other way as may be practicable—the construction of gun-pits, I say, at so insignificant a cost, and under so many various circumstances of locality, or especial requirement, appears to me to be of very great importance; and in this light, as an engineer, I view the proposal with especial interest. As regards the application of the system to fortifications, that is to constructions having a command over the country, certain difficulties are introduced which have not, I think, been altogether appreciated. The penetrating power of the guns of recent construction is so great that, for parapets, if constructed of earth, a thickness of 30 feet is necessary:* that is a thickness which renders an embrasure almost impracticable. You could not place a gun in the embrasure of a parapet 30 feet in thickness with any certainty of efficiency. But it may be said that parapets will not be constructed of earth; still they must be constructed of some material which will require a sufficient thickness to make them shot proof, and they involve considerations which have not been referred to by those who have favoured us with their views on this subject, but which will form very important points in the construction of rampart batteries on this principle. They are not difficulties which cannot be overcome, but they are difficulties which will require attention and care. With these remarks, I think I may offer Captain Moncrieff our thanks for the admirable paper which he has read to us this evening upon the gun-pit system. I do hope that by perseverance, and by pursuing his arguments in that modest, yet complete, manner which has distinguished his papers in this Institution, in setting before the authorities the importance of his invention, Captain Moncrieff will succeed in inducing the Government to accept the principle as one worthy of being practically tested; and I am fully satisfied that whenever it may be adopted, it will be found pregnant with important results, and Captain Moncrieff will be awarded the credit which is justly his due for having brought the subject forward, and pressed it to a practical issue.

---

* See a paper by Captain W. S. Boileau, R.E., "Notes on the Comparative Penetration of Shot and Shell from Rifle and Smooth-bored Ordnance into Natural and Made Earthworks," Journal of the Royal United Service Institution, vol. ix, page 395.—Ed.

Evening Meeting.

Monday, May 6th, 1867.

MAJOR-GENERAL J. T. BOILEAU, R.E., F.R.S., in the Chair.

NAME of MEMBER who joined the Institution between the 1st and 6th May, 1867.

ANNUAL.

Wilson, Edmund, Captain R.N. 1*l*.

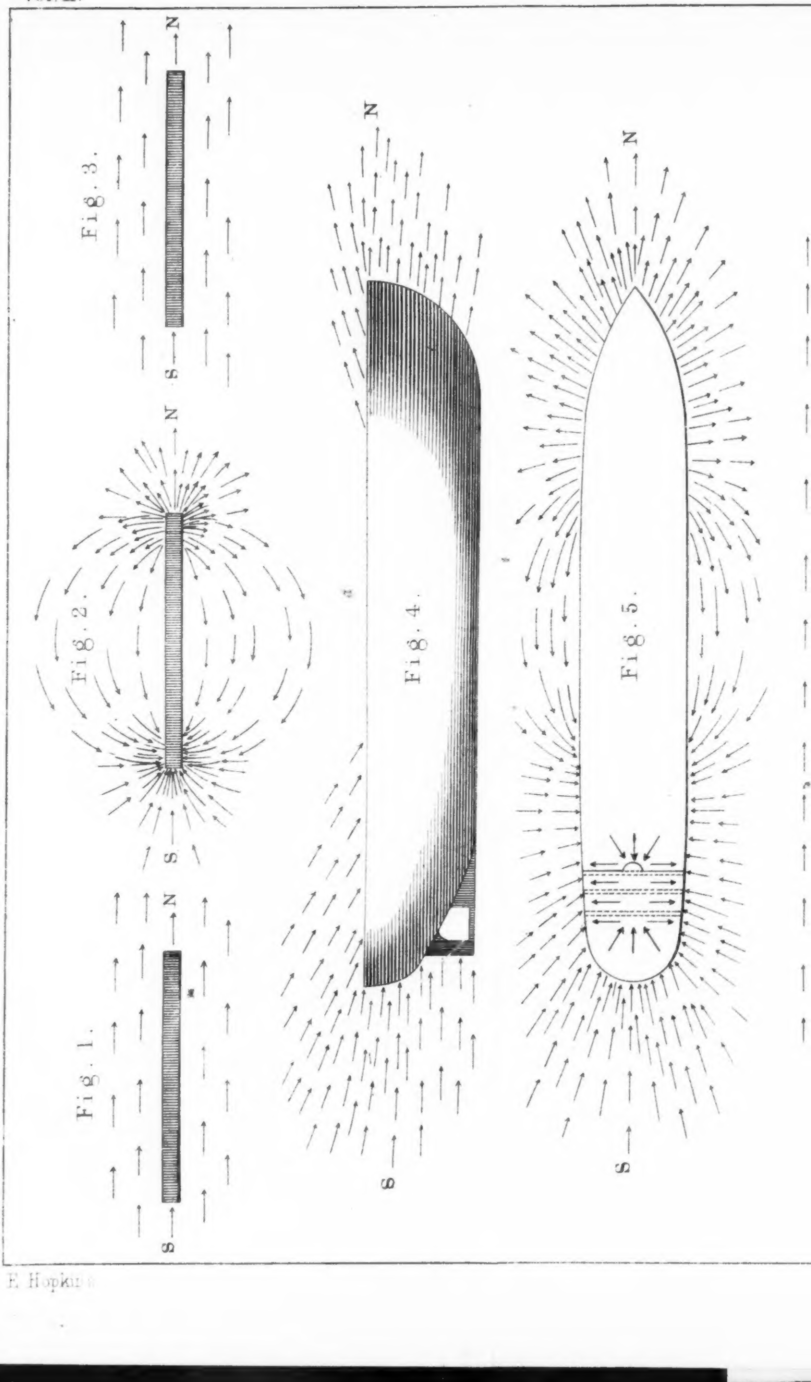
THE DE-MAGNETIZATION OF IRON SHIPS, AND OF THE IRON BEAMS, ETC., OF WOODEN VESSELS, TO PREVENT THE DEVIATION OF THE COMPASSES, EXPERIMENTALLY SHOWN BY MEANS OF A MODEL.

By EVAN HOPKINS, C.E., F.G.S.

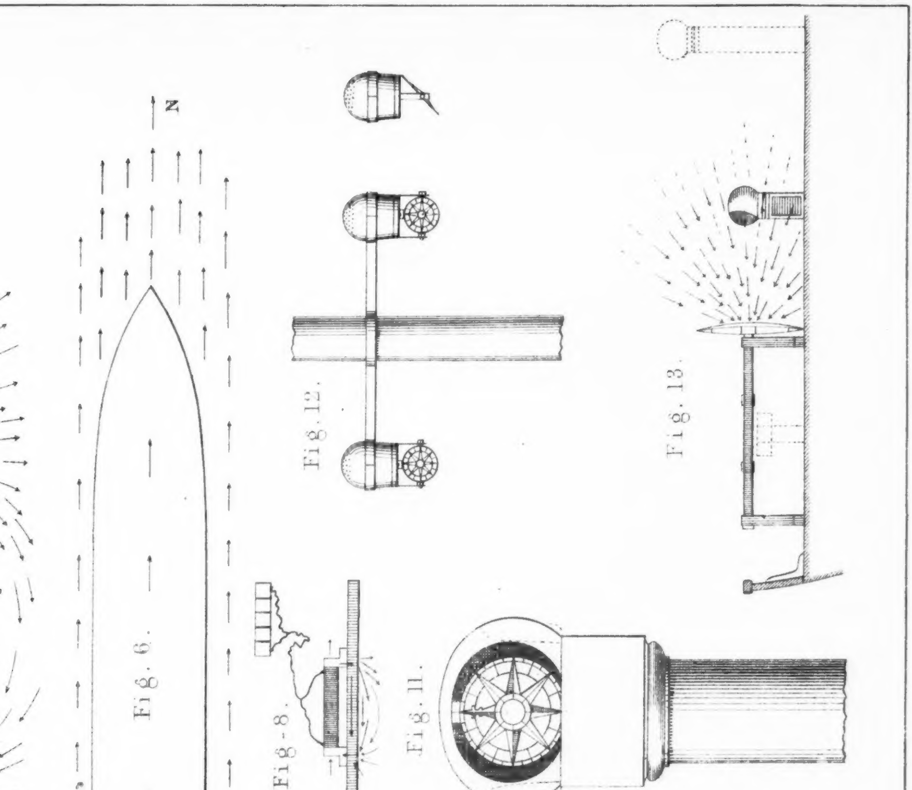
Read by his son, the Rev. E. HOPKINS.

THE magnetism of iron ships, and the embarrassments which it occasions in their navigation, in consequence of the deviations of the compasses, have been lately so fully entered into in the very able and elaborate papers read before this Institution by Staff Commander Evans, as to render it unnecessary to enter minutely into the subject. I shall simply draw attention to some of the leading facts, with a view of showing the present state of this question. Notwithstanding the considerable attention which the subject has received, it is admitted that mariners are still surrounded by difficulties which none of the systems of correction at present in use, can remove. Indeed nothing short of the absolute de-magnetization of the iron affecting the compasses can effectually remove the evils resulting from such an influence, which not only causes very serious errors in their direction, but also reduces their directive power, and thereby increases the heeling errors when sailing in the meridian.

Until lately the system of ascertaining and correcting the errors of the compasses in the Royal Navy, was somewhat different to that adopted in the mercantile marine, as explained in my former paper, but in consequence of the increased amount of iron introduced in the construction of the iron-plated ships, the compass deviation has become



E. Hopkins



J Jobbins.

so large, that the employment of magnets to reduce the amount of deviation has become unavoidable; but the method of correction by magnets, however perfect, is not considered as superseding the construction of a table of errors, by means of swinging, and navigating the ship by that table. The system of having a standard compass and a table of deviations is adopted in the merchant service, as well as in the Royal Navy: therefore as regards the embarrassments attending the navigation of iron ships in thick weather, they are more or less alike in both services.

Again, as regards the compasses themselves, and the position of the binnacles on board iron ships, the Admiralty Instructions are as good as could be wished, but unfortunately we find these instructions frequently as much disregarded in some of the ships of Her Majesty's Navy as in the mercantile marine. No system of correction or remedies could be rendered of any service, if compasses be placed so near iron beams as to prevent them from acting freely.

In my former paper I remarked that—"Many of the errors of devia-"tion attributed to the hulls of the ships may be referred to the mag-"netism of the steering gear and the deck beams, and the defective "manner in which the binnacles are often placed on board iron ships." This fact has been subsequently verified by observations made on board several vessels in the London docks; it therefore follows that, however free the hull may be from any magnetic influence, the compass may still have a considerable deviation, arising from the magnetism of the beams in its immediate vicinity, which magnetism must necessarily be destroyed before the compass can possibly act correctly. We have recently had an example of this kind of disturbing influence from internal masses of iron in H.M.S. "Dryad," a wooden vessel with iron beams. This ship, on her return from a short trial near the entrance of Plymouth harbour, had a narrow escape of being wrecked, owing to the *magnetism of her beams*, which caused a deviation in her compasses of $5\frac{1}{2}$ pts., or say $60^\circ\ 30'$. Indeed, in the majority of cases in the larger vessels, the principal disturbance proceeds from the effects of local masses of iron, and not so much from the hull, if the compass be 20 feet from the sides.

Before I proceed to shew how the hull of any given vessel, or the beams, or any other masses of iron on board, which may disturb the compasses, are to be de-magnetized, I shall first give a brief description of the method, so that the system may be thoroughly understood.

The *de-magnetizing* process is effected on the same principle as that adopted in destroying the magnetism of steel-bar magnets, when they are no longer required to be employed as magnets. As this principle may not be generally known amongst those who have not studied the science of magnetism practically, I shall give a brief description of the process by shewing how magnetic bars are magnetized and de-magnetized by means of a horse-shoe magnet. Fig. 1 represents a prepared steel bar, tempered, but not yet magnetized. If we place small compasses on both sides and within a short distance of this bar, they will not be disturbed, or made to deviate from their meridional direction, and will

retain their parallelism, as shewn by the arrows. Steel and iron bars, sewing needles, &c., in their normal state, may be placed together without indicating any tendency to attract, or adhere to, each other; but the moment they are magnetized they will attract and unite together into one bundle. This magnetic power of attraction will remain if not destroyed; but if the separate parts be de-magnetized, they will be restored to their former normal or neutral state. If we pass over the bar (figure 1), a horse-shoe magnet from S. to N., with the S. end of the magnet forward towards the intended north end of the bar, so that the magnetic current may pass through the bar from south to north, the bar will acquire polarity; the intensity of this polarity will depend on the power of the magnet, and the number of times it is made to pass over the bar from S. to N. The moment the bar acquires this polarity it will affect the compasses placed in its neighbourhood to a considerable distance, and make the direction of the needles to conform to the magnetic curves, shewn in fig. 2.

If the bar be made of steel, and properly tempered, it will retain this magnetic power permanently. If however it be only a hard iron bar or beam, such as iron ships are made of, the magnetism will become gradually less intense in the course of years; but nevertheless will continue to be of sufficient intensity to cause a great deviation of the compass placed in its vicinity. Therefore it is indispensably necessary to remove this magnetic power; that is to de-magnetize the iron ship, the iron beams, or other disturbing masses of iron, so that the compass may be allowed to act correctly and without deviation.

This highly important object can now be easily and effectually done in the following manner:—

If we refer back to figures 1 and 2 and see how the bar first acquired its magnetism, viz., by means of passing currents through it from S. to N., we may reasonably conclude that if we pass currents through it in a contrary direction, we shall destroy its magnetic power. By reversing the horse-shoe magnet so that we pass it now in the opposite direction, *i.e.*, from N. to S., the polarity of the bar will be destroyed; and if the operation be carefully done, the bar will be restored to its original normal condition, as shown in figure 3. It must be borne in mind that if this contrary action be continued longer than is necessary to destroy its previous polarity, it will acquire a contrary polarity. If however this opposite effect be produced, it may be easily removed by reversing the operation. This is precisely the system I adopt in de-magnetizing on a large scale; whether I operate on the hull of the ship, or on the individual iron beams and other masses of iron near the compasses; and whether I employ ordinary horse-shoe magnets or electro-magnets.

Before commencing the operation, it is necessary that the general character of the magnetism of the hull should be correctly ascertained, and the direction of the magnetic currents of the iron beams accurately noted, as it is evident, that upon the nature of this acquired polarity, the particular course to be pursued in the process of de-magnetizing, will entirely depend.

I will now explain by means of the model, the experiments made on

board H.M.S. "Northumberland," and furnish a table of the results as far as I was able to carry out those experiments. They were conducted at Sheerness in January last. The weather was extremely unfavourable and very cold, and I was at the time suffering from a very severe illness, which completely prostrated me before I was able to complete the operation. In spite, however, of these difficulties, I am happy to state that the de-magnetization was carried on sufficiently far to prove the perfect practicability of the process, which was the main object of the experiment.

The "Northumberland," had acquired during construction a very strong polarity, in accordance with her position on the stocks. The bow was the north pole, from which magnetic lines radiated outwards from 30 to 60 feet, and the same at the stern as represented in figure 5.

I shall first give the model the same kind of magnetism, in order to show the effect experimentally, and how the hull was de-magnetized.

The deviation of the compass at the place occupied by the poop binnacle on the "Northumberland" was 90° before launching. This is shown on the model, which on being swung while it has this polarity will indicate deviations of the compass placed on the poop, of the following character:—When the head of the vessel has a westerly bearing, the deviation will be east; and when the ship's head is east, the deviation will be west, showing that the north end of the needle is attracted by the stern, or south pole of the ship. In like manner, when the "Northumberland" was swung at Sheerness, the poop compass had a maximum deviation of 53° E. when her head was west, and a corresponding westerly deviation when her head was east, *i.e.*, the north end of the needle was attracted to the stern.

These facts confirm the law originally observed by Dr. Scoresby, and subsequently by the Compass Committee of Liverpool, and noticed by Commander Evans, in his last paper read at this Institution, as follows:—

"The remarkable law of the direction of the force causing the "semicircular deviation in iron-built ships being dependent on their "position in building, was established prior to the application of armour "plates to ships of war. This law, provided the compass is free from "the influence of individual masses of iron, may be thus expressed, "'*the north point of the needle is attracted to the part of the ship which* "'*was south in building.*'"

Having shown the effect of the acquired polarity on the compass, I shall now proceed to destroy it by de-magnetizing the hull. This is effected by passing two horse-shoe magnets along the sides from end to end, in a contrary direction to its polarity, as will be observed. On re-swinging the model it will now be observed that the polarity has been entirely removed, and the compass will now point correctly at all the bearings, without indicating any deviation.

On the "Northumberland," instead of horse-shoe magnets, which are not sufficiently powerful for the immense mass of iron to be operated upon, strong electro-magnets were employed, with two Grove's batteries of five cells each, and passed from end to end in a direction contrary to the acquired polarity, in the same manner as

shown on the model. This destroyed the polarity of the hull, and affected compasses, placed within a distance of from 8 to 10 feet from the side. Now, had the "Northumberland" consisted of nothing more than a mere iron shell—like the model—or had the binnacles been placed at distances sufficiently far from the magnetism of individual masses of iron beams, etc., then on the polarity of the hull being destroyed, the whole of the compass deviation would have been removed—in the same manner as is done in the model. But as the vessel contains a number of strong iron beams, which had acquired a high degree of magnetism, and within 3 feet 8 inches of which the cards of the binnacles were placed, it is quite evident that these beams, as well as the hull of the ship, had to be de-magnetized.

I shall further explain this by means of an experiment on the model.

The magnetism of the poop and deck beams was first carefully examined before they were touched by the electro-magnets, in order to ascertain the direction in which the operation was to be made in order to produce the desired effect. In the bearing in which the ship then stood, the deviation of the poop compass was about 37° E. The moment the depolarizers (electro-magnets) were passed over two of the beams underneath and near the binnacles, the deviation was reduced to 5° E. I then discovered that the beam on which the poop compass was placed, and immediately under which the two binnacles were situated, was so strongly magnetic that it was the principal cause of the disturbance, and that, therefore, it was necessary that it should be de-magnetized. This beam was unfortunately lined or covered with wood, and could not be properly dealt with, without removing this lining. A part of this was taken off, and the beam was partially operated upon, but not sufficient effect was produced, owing to the above reason, to de-magnetize it entirely.

This inconvenience, together with the severity of the weather and the complete prostration to which I was reduced by illness, rendered it impossible for me to proceed further in removing the magnetism of the iron which affected the compasses. What had been effected, however, was sufficient to prove the complete efficacy of my plan, and that the magnetism of the largest, as well as of the smallest iron ships, can be effectually destroyed.

The steering gear of the "Northumberland" is constructed of brass, to prevent any influence on the binnacles from this source; but this precaution is of little avail, if the iron beams, above and below the compasses, and within so short a distance of them, are allowed to remain in a highly magnetic state. The table, at the end of this paper, shows the degree to which the deviation of the poop and port compasses was reduced on the last day I was able to be on board.

The binnacles should be properly placed at the required distance from iron, and all the beams or masses of iron that have acquired polarity, should be left bare so that their magnetism may be destroyed, and the compasses rendered completely free from all disturbances. There is scarcely any *soft* iron in the frame of an iron ship. It is composed of iron of various degrees of *hardness*, which does not ac-

quire magnetism rapidly, like soft iron, but when once it becomes saturated with the magnetic power, it will retain it for many years; though its intensity becomes gradually less and less every year, as it is not capable of preserving it uniformly and permanently, like hardened steel. For this very same reason, after the magnetism of the *hard iron* has been removed, it will not return again, unless the ship be exposed to hammering and reconstruction. The retentive power of the *hard* iron of the plates, ribs, and beams of iron ships, has been proved in the case of the "Great Britain" and several other ships, which could be mentioned. During the first few months after launching, a very large proportion of the acquired magnetism, vanishes. As to the alleged effects of the action of the waves on the hull, the concussion and vibration thereby produced, may tend to hasten the gradual weakening of the ship's polarity, but never to *change or reverse* it. Were this the case, an iron ship's compasses would be constantly deviating to the east and to the west on the same course during a short voyage in stormy weather. Those who entertain such ideas could never have made observations on the magnetism of ships at sea. The actual change as already stated is merely the *gradual diminution* of the intensity of the polarity of the hull acquired during construction, and not the *reversal* of the poles. Hence when once the polarity, or the magnetism, of an iron ship is destroyed, it will not return, and the hull will have as little effect on the compass as that of an ordinary Thames steamer.

Although the experiments on board the "Northumberland" were not made under the auspices of the Admiralty, but simply by their permission, yet it is my duty to acknowledge that I have received the greatest courtesy from their Lordships. When I proceeded to Sheerness, in January, to make the experiments, they instructed their Naval Officers to afford me every facility in carrying out the process on board the "Northumberland." I met with the greatest attention from all the Naval Officers. They feel a very great interest in the invention, and are looking forward to be relieved of the embarrassments and risks attending the present system of correcting the deviations of the compasses. In spite of the difficulties already referred to, the severity of the weather and my severe illness, I was enabled to carry the operation sufficiently far to prove the perfect practicability of the de-magnetizing process. I intended not only to complete the "Northumberland," but also to de-magnetize two or three other vessels then in the harbour. I had not the pleasure of seeing any of the members of the Compass Department on board the "Northumberland." I have also to regret, that some of its members are much displeased at their Lordships granting me such privileges, and in feeling such an interest in the de-magnetizing process. They privately maintained that I could not effect the object; but now seeing the effect produced on the poop-compass of the "Northumberland," which they acknowledged in the following terms:—"The maximum deviation has been reduced from 54° to 17°," and seeing that I could easily take away the remaining 17°, they recommended their Lordships that in the next ship I should not be allowed to touch the beams! But these compass errors, and the

daily increasing dangers resulting from them, are of much too serious a character to be trifled with. Some more practical and efficacious method, than that now in use, to render the compasses of iron ships trustworthy, is urgently called for, quite as much in the Navy as in the mercantile marine. If, therefore, any fatal wrecks should occur (which indeed may happen any day), and it can be proved that they arose from compass errors, and that these errors could have been removed by a simple and effective process, those who resist such a remedy in ships under their charge, will do so at their own peril.

In the memorandum addressed to the Board of Trade, with reference to the magnetism of iron ships is the following remark:—

" The President and Council (of the Royal Society) think it right to " call the attention of the Board of Trade to the serious responsibility " they incur in cases of loss of life and property, arising from the want " of a proper system of compass adjustment, by declining to take the " course which is pointed out." " They cannot but look forward to a " time when the necessity of a proper supervision will be forced on " the Executive by public feeling, excited by some disastrous loss of " human life, traceable to the want of such superintendence."

If the Board of Trade incur a serious responsibility, because they decline to interfere with the supervision of the compasses of the mercantile marine, and without having been furnished with any method by which such evils can be removed, how much more serious must be the responsibility of the Compass Department of the Admiralty in the event of any of H.M. ships meeting with such disasters, should its members resist the introduction of a process which effectually and permanently affects the object. The Admiralty are the owners, designers, and generally the builders of the ships of the nation, and are bound to use every available means in their power to ensure the safety of their navigation. The Board of Trade has no right to interfere with the building and equipment of merchant ships; if therefore an efficient practical remedy be discovered by which the compass errors can be removed, in case of fatal casualties the responsibility will rest on the owners of the ships, and on them alone. Even had the Board of Trade received a satisfactory reply to their application for an efficient practical remedy which could be immediately applied, instead of the mere supervision, and the tentative and experimental process suggested, they could only recommend it to the consideration of shipowners, and remind them of the responsibilities incurred in cases of fatal shipwrecks, traceable to causes arising from the neglect of available remedies. It is known to many, that iron ships are sent to sea in the most dangerous state as regards the condition of their compasses, owing to the disturbing influence of the magnetism of their hulls and beams, especially when new.

In the coal fleet of the north, many disasters occur in consequence of this evil, but which on being recorded, are attributed to the neglect of using the lead. Numerous casualties have occurred in the channels, and several vessels during the last few years have been wrecked, owing to the great deviations of their compasses. Although these facts are now pretty well known, it is supposed that they are necessary

and incurable evils, inseparable from iron ships, and to which we must become reconciled, as they cannot be removed.

Shipowners, therefore, pay little attention to the subject, and protect themselves by means of their floating policy. The Captains of iron ships are now expected to ignore the use of the compasses in thick weather and in channels, and be guided solely by soundings. Hence in all the recent cases of casualties, some of which were fatal, and which were primarily caused by the deviations of the compasses, and the magnetism of the beams, the captains were reprimanded, or their certificates suspended, because they did not ignore the use of the compass and depend solely upon the soundings of the lead. The public at large are thus kept almost in the dark with regard to the real causes of many of these wrecks. Nothing can excuse the neglect of all precautionary measures to ensure safety on board a ship: but surely such an indispensable and highly important instrument as the compass should have all the attention necessary to render it accurate and trustworthy, and to avoid the possibility of its becoming treacherous and misleading, from the disturbing influence of the ship's magnetism, and its being placed in an improper position.

It behoves Commanders of iron vessels to look after their own interest and the safety of themselves, and the lives of their crew. While shipowners can protect their own interest by their floating policy, however great and irregular the errors of the compasses may be, it is not probable that they will trouble themselves about compasses, or the magnetism of their ships. The lives of crews and passengers are frequently sacrificed in shipwrecks primarily caused by the errors of the compasses, but generally attributed to the want of using the lead, or some other neglect on the part of the officers.

Having studied this subject for many years, and knowing its vital importance to the safety of the navigation of iron ships, I have spared neither time nor expense in experimenting on and bringing the de-magnetizing process to a simple but efficacious and practical issue, so that it may be immediately adopted in the Navy and the mercantile marine.

When furnishing to the Admiralty the particulars of the experiments on the "Northumberland," I mentioned that having satisfied myself of the perfect efficiency of the process, I was now prepared to undertake the de-magnetizing of all the iron ships of Her Majesty's Navy; and also, if required, the training of a staff of Naval Officers to carry out the system.

Reference to the Figures on the Plate.

Figs. 1, 2, 3. *Steel bars* (in a neutral and magnetic state).
Figs. 4, 5, 6. Ship's hull (in a magnetic and neutral state).
Fig. 7. Horse-shoe magnet.
Fig. 8. Electro-magnetic.
Figs. 9, 10, 11, 12. Compasses with reflecting cards.
Fig. 13. Steering axle highly magnetized.

A TABLE showing the deviations of the Poop and Port Compasses of H.M.S. "Northumberland," before and after depolarization, as far as the process was carried on, 18th January, 1867. Sheerness.

Direction of the Ship's Head.	Original deviation of the Poop Compass.	Reduction by Depolarization. Poop Compass.	Original deviation of the Port Compass.	Reduction by Depolarization. Port Compass.	Remarks.
	° ′	° ′	° ′	° ′	
North	16 0 E.	5 0 E.	20 0 E.	6 0 W.	Columns 3 and 5 show
N. by E.	7 0 ,,	6 0 ,,	12 0 ,,	5 40 ,,	the progress made in
N.N.E.	1 30 W.	7 0 ,,	6 0 ,,	5 0 ,,	reducing the devia-
N.E. by N.	10 0 ,,	7 0 ,,	2 0 W.	4 30 ,,	tion. More than half
N.E.	18 20 ,,	6 0 ,,	9 0 ,,	3 0 ,,	of this reduced de-
N.E. by E.	25 20 ,,	4 0 ,,	17 0 ,,	3 0 ,,	viation is owing to
E.N.E.	32 40 ,,	3 0 ,,	25 0 ,,	3 0 ,,	the binnacles being
E. by N.	39 0 ,,	0 0	31 0 ,,	2 0 ,,	placed *too near* a
East	44 30 ,,	0 0	39 0 ,,	0 0	highly magnetized
E. by S.	48 40 ,,	0 0	44 0 ,,	0 0	beam in front of the
E.S.E.	52 0 ,,	3 0 W.	49 0 ,,	0 0	poop deck, which was
S.E. by E.	53 0 ,,	6 0 ,,	48 0 ,,	0 0	lined with wood, and
S.E.	52 0 ,,	9 0 ,,	47 0 ,,	2 0 ,,	could not be easily
S.E. by S.	48 20 ,,	10 0 ,,	45 0 ,,	0 0	operated upon with-
S.S.E.	40 0 ,,	10 0 ,,	39 0 ,,	0 0	out its being un-
E. by S.	26 40 ,,	10 0 ,,	30 0 ,,	3 0 E.	covered. The cards
South	12 40 ,,	8 40 ,,	20 0 ,,	8 0 ,,	of the compasses
S. by W.	0 30 E.	7 0 ,,	8 0 ,,	10 0 ,,	should never be
S.S.W.	12 0 ,,	5 0 ,,	5 0 E.	12 0 ,,	placed so near iron as
S.W. by S.	28 40 ,,	3 0 ,,	16 0 ,,	13 0 ,,	3 feet 8 inches. They
S.W.	31 40 ,,	0 0	26 0 ,,	12 40 ,,	should be placed ac-
S.W. by W.	38 0 ,,	0 0	33 0 ,,	12 0 ,,	cording to the Ad-
W.S.W.	42 40 ,,	4 0 E.	36 0 ,,	10 40 ,,	miralty Regulations,
W. by S.	47 0 ,,	7 0 ,,	38 30 ,,	7 0 ,,	about 7 feet from iron.
West	49 0 ,,	8 0 ,,	40 40 ,,	4 0 ,,	The final destruction
W. by N.	49 20 ,,	7 0 ,,	42 30 ,,	0 0	of the acquired mag-
W.N.W.	48 0 ,,	6 30 ,,	42 20 ,,	0 0	netism requires to be
N.W. by W.	46 0 ,,	6 30 ,,	43 0 ,,	4 0 W.	done with care, to
N.W.	42 0 ,,	6 30 ,,	42 0 ,,	6 0 ,,	prevent a contrary
N.W. by N.	37 20 ,,	6 20 ,,	32 40 ,,	7 0 ,,	polarity being given
N.N.W.	30 40 ,,	6 0 ,,	32 0 ,,	8 0 ,,	to the iron.
N. by W.	23 30 ,,	5 30 ,,	26 20 ,,	7 0 ,,	

The CHAIRMAN: This question, viz., that of "the de-magnetizing of iron vessels" is one which affects deeply the general welfare of all who embark on board such vessels. It is one which has many points of view, and I dare say some gentlemen here will be glad to offer observations upon the subject, and to raise a discussion, which I hope will be of general use in its bearings upon the very important and interesting question now before us.

Captain INGRAM, R.N.: Will the de-magnetized condition of the vessel continue south of the line?

Mr. HOPKINS, jun.: Yes; we de-magnetize once for all. The polarity never returns after the vessels have been once de-magnetized.

Captain INGRAM: If we swing a ship in our latitudes, and then go south of the

line, the effect will be reversed. I had the honor of commanding an iron ship, the "Birkenhead," and our greatest deviation was not more than two points, though we had not been de-magnetized.

Mr. HOPKINS, jun.: We could easily take all the magnetism away.

Admiral RYDER: There is one point which Mr. Hopkins has not touched upon, but which will make his system of great advantage to us if it should prove to be perfectly efficacious. I refer to the chance of deviation in an iron ship when she is rolling. I believe that all the methods we have in use at present are fallacious when a ship is rolling. The corrections which we ascertain when a ship is on an even keel are all wrong when the ship rolls; whereas, if Mr. Hopkins succeeds in de-polarizing the ship altogether, his method will be as useful when the vessel is rolling, as when she is on an even keel. I should like also to ask, did Mr. Hopkins ascertain how many beams would have to be de-polarized on board the "Northumberland," did he operate on them all at the same time, and was it a part of the process that the de-polarizing operation should be performed upon all the beams at the *same* time?

Mr. HOPKINS, jun.: There were about six.

Admiral RYDER: Did he operate upon them all at the same time?

Mr. HOPKINS, jun.: Yes. That was part of the process.

Admiral RYDER: You could thus see how you were succeeding, and when you were to stop?

Mr. HOPKINS, jun.: Quite so.

Mr. GROVER, C.E.: There is one question I should like to ask, viz., how this operation of de-magnetizing is practically carried out. We all understand the principle, but is it necessary to employ a number of men to move these magnets? Do they hold on with much strength? Was it an expensive or laborious operation, or very simple? And to what extent had you to extend the operation? Along the whole side from stem to stern?

The CHAIRMAN: Our usual practice is for the gentleman who has read a paper, to take notes of questions that are asked him, and to answer them at the close of the discussion.

Mr. VIGNOLES, C.E., F.R.S.: I think the gentleman on my left (Captain Ingram) observed that the plan now in use answered pretty well, within one or two degrees.

Captain SELWYN: In one particular instance; in that particular ship only.

Mr. HOPKINS, jun.: On a ship the "Birkenhead"—two points.

Mr. VIGNOLES: I beg your pardon, I thought you said two degrees. However, what I want to draw attention to, is this:—By the present mode of correcting the compasses by swinging the vessel, a certain correction is produced; and I understand the gentleman on my left (Admiral Ryder) to say that when the ship rolls very much that is entirely lost.

Admiral RYDER: No, not entirely.

The CHAIRMAN: It is exaggerated.

Mr. VIGNOLES: It is exaggerated. Then, if this process, which has been so simply and so accurately described, and which I think we all understand, has the effect of neutralizing or de-polarizing the iron in the ship, of course, whatever roll that ship will hereafter take, it will be almost self-evident that it would not cause the compass to deviate. If the present polarity of the vessel causes the deviation of the compass so completely, and there is only partial correction obtained by swinging the vessel—

Captain FISHBOURNE: There is no correction obtained. There is only a table of errors; there is no correction.

Mr. VIGNOLES: It is the same thing?

The CHAIRMAN: You ascertain the effect upon the compass.

Mr. VIGNOLES: Exactly. But that table is not to be depended upon, you say, when the vessel rolls. (The CHAIRMAN: No.) Then our course appears to me to be exceedingly clear. If this process which Mr. Hopkins has described is effectual, it would require but a few weeks to set a vessel right. (Captain FISHBOURNE: Quite so.) If it is so simple as he says it is, surely the Royal Society, or all the Societies of England would join in pressing upon the Government, the necessity of

having the experiment fairly carried out. (Captain FISHBOURNE: That ought to be so.) It is not an experiment which is expensive, and it is a simple one. All that is required is that the vessel to be acted upon shall be thoroughly de-polarized, and then subjected to these experiments.

Captain FISHBOURNE, R.N.: I will endeavour to explain the effects that Admiral Ryder referred to, and why the present system is inefficacious; that is to say, why the corrections are of comparatively little value. Perhaps, however, it is too strong an expression to say that they are not correct; but they do not give a true estimate of the deviations when the ship is rolling. You will have observed, that when the beams were polarized or magnetized, they affected the compass, that is to say, they caused the compass to deviate. Now, take the case of the "Northumberland." It is stated, in a letter from Captain Evans, that the deviation of the "Northumberland's" compasses was, of one, 53, and of the other, 51 degrees. You can understand that if the compass is deflected from its true direction, 45 degrees, the directive force of the compass needle will be reduced one-half. The consequence of that is, that the power which the needle has to retain its position constantly pointing to the north, is reduced by one-half. If the deviation, as was the case in the "Northumberland" at launching, was 90, the needle had no directive force at all to point to the north. Upon setting it oscillating, it would oscillate a length of time. Now, in the case of the "Northumberland," when she is rolling, the directive force of the needle is reduced so much, that it has no power to retain its position, and it moves round with the ship. If the ship goes round, the friction of the pivot will carry the compass with it, because there is no force in the needle to maintain its position constantly; yet here we have it argued by Captain Evans that it is a matter of no moment.

Rear-Admiral Sir FREDERICK NICOLSON, Bart., C.B.: May I ask what that document is?

Captain FISHBOURNE: It is a letter which Captain Evans wrote to the *Athenæum*, in which he denied that Mr. Hopkins had reduced the deviation of the "Northumberland" at all.

The CHAIRMAN: What is the date?

Captain FISHBOURNE: The date is in September, 1866. The fact being, as Mr. Hopkins tells you in his paper, that if the compass placed on the centre line of the ship, be 30 feet from the iron skin, it will not be affected to a large degree, and will not be affected at all by this coating, if there are beams which are magnetized to any great amount. Mr. Hopkins de-polarized the external surface of the "Northumberland." Her compasses were not in their places, but compasses placed on the outside showed that the needles ranged themselves first parallel to the keel of the ship. After the process was complete, they pointed to the pole; previously they had been indicating 90 degrees deviation from the pole.

Admiral RYDER: Was her head north and her stern south?

Captain FISHBOURNE: As she lay in dock.

Mr. HOPKINS, jun.: Just the reverse.

Captain FISHBOURNE: It is not stated here. I take it as I find it. It is admitted by Captain Evans in his Report to the Admiralty that Mr. Hopkins had reduced the deviation from 53 to 17 degrees. Now, you will observe that if the 45 degrees of deviation reduce the directive force of the needle one-half, if Mr. Hopkins reduces the deviation two-thirds, he adds two-thirds of that force to the effective value of the compass needle. No doubt when the ship is rolling, it will not indicate quite correctly; but it will indicate more correctly, because of the directive force being increased by that large amount. If, on the contrary, Mr. Hopkins had been allowed to reduce the whole of that 17 degrees, he would have doubled the directive force of the needle. Now, you can understand, when a ship is swinging round fast, and in a narrow channel, how important it is that the compass shall always indicate truly, and that it shall not travel round with the ship; that you shall be able to ascertain at any moment what is the true position of the ship's head, how many points she has deviated from the point you start from. You cannot have that, where you have compasses so influenced by local attraction that they have little or no directive force at all. Therefore, so far from there being

any force in the argument of Captain Evans, it is the very reverse. Then he says he would prefer as an alternative, magnets, although he had previously deprecated the use of the magnet (the magnet acting just like these beams—decreasing the directive force). Thus, not only would he not have the ship de-magnetized, the effect of which operation is to increase the directive force, but he would add to the evil; he would multiply it, because he would introduce magnets that would still detract from the directive power of the needle. In point of fact, he would reduce the directive power of the needle to *nil*. So you would find it would oscillate. You will see it here now.

(Mr. HOPKINS, jun., proceeded to show the experiment.)

Admiral RYDER: May I ask Mr. Hopkins what he has done?

Mr. HOPKINS, jun.: I have polarized the model.

Captain FISHBOURNE: He has polarized it, the object being to reduce the effective force of the needle.

Admiral RYDER: Has he put the compasses as they were in the "Northumberland."

Captain FISHBOURNE: He has merely put one. You observe how it oscillates; and if you take it away, you will find how the directive power of the needle towards the north increases, and the oscillations stop; that is to say, it will indicate truly any motion of the ship's head. Its power will be so great that though the ship is rolling, still the force of magnetism will be greater than the mechanical force of the ship, that is to say, the mechanical force through the pivot: the needle will retain its position, it will indicate truly, and you will know what you are about. It is not simply that, however; you have not got a constant quantity to deal with. If you knew your errors, or always measured your errors, you would have some guide; but you have not. From the moment the ship is launched there is a diminution of the magnetism of the ship going on, and it is going on for years. And that is not enough; it is an unequal quantity; you cannot measure it. It varies with the nearness of the magnets to the iron; it varies with the course you steer, with the rapidity of change in the course. These are really all alterations which you cannot provide for. Then, again, the magnets themselves are subject to diminution of power. Placed as they are at right angles to each other, they all three operate to decrease their own magnetic power, and to increase the evil. Common sense dictated at once the sweeping the whole of these away. I do protest against the course that has been taken in opposition in this matter. When a gentleman comes here, takes time about a public question, and offers to exhibit his process in the face of any one in committee, or to explain it to any one, I do protest against subordinates setting themselves up in opposition. We hear that the Admiralty themselves are not opposed to it. They give every facility that is wished for. The Officer under whose eye the experiment was carried on, Captain Cochrane, told me that Mr. Hopkins was so ill that he (Captain Cochrane) told him that he would kill himself: the severity of the weather was so great. Yet what is the fact? When the Report upon this experiment is made, what is it? Instead of allowance being made for this gentleman's illness, it is stated that he was eighteen days performing the operation. Now, I want to know, on a first experiment of such magnitude as this, and on so important a subject as this, when he was operating a great deal to acquire knowledge, why the question of time is to be considered at all? But I will not deal with this. I will just state the facts, and leave you to judge for yourselves. Mr. Hopkins has never recovered himself, and I doubt if he is likely to recover. Captain Cochrane told me he thought he would have killed himself if he had continued his labours.* He remonstrated with him upon the subject; but Mr. Hopkins felt the importance of his experiments, and he thought he ought to continue them. But instead of allowance being made for his illness, instead of allowance being made for the severity of the weather, instead of allowance being made for the fact that he was only able to work three or four hours a day, in this report it is objected that he was eighteen whole days employed upon it. If you take the working hours employed, it would have amounted to three days. But if it had been thirty days, it has nothing to do with the scientific bearings of the question. I really put it to you—does it not

* Mr. Hopkins has since died.—ED.

mark the utter want of confidence they have in their own opposition when they make statements of that description? I do protest against them. I do sympathise entirely with Mr. Hopkins in his illness, and I think it bespeaks our consideration. I perhaps may have spoken warmly upon the subject; but I do think that public affairs should not be so trifled with, and men's interests so unfairly prejudiced.

Sir FREDERICK NICOLSON: I am anxious to say a few words upon this question. I think it is a matter of the greatest possible regret that Mr. Hopkins is not here himself, as he would have been enabled to give us some more detailed information with regard to the effects upon all the compasses of the "Northumberland." As far as I have heard, the results of his first operations, which, I think, took place in April, or somewhere about then, were that no perceptible effect was produced. Those operations were only on the hull, I think. Am I right. [Mr. HOPKINS, junr.: Quite right.] And they had no appreciable effect upon the compasses. I do not know precisely all that has taken place, since I believe there has been a long correspondence upon the subject. Into that I do not enter, because really I am imperfectly acquainted with it. But, subsequently, Mr. Hopkins received permission from the Admiralty to operate upon the "Northumberland" at Sheerness. I think, as Captain Fishbourne truly said, it shows Mr. Hopkins' great zeal in this cause, that in that terrible weather he operated upon that ship at Sheerness. It must have been a frightful task for Mr. Hopkins. But I think all that we have to consider here, as practical men, is what are the results that have been obtained from those operations? Now, I have not seen any detailed statement of the observations made by the Compass Department subsequently to the operation of Mr. Hopkins. If those operations, on the whole, had no more effect than they had before, in consequence, as Mr. Hopkins stated to us to-night, of the compasses being at too great a distance from the sides of the ship for those operations to have any effect, then I think, so far, the effects are still nothing. But when we come to the other operations on the beams, all, who have even the slightest acquaintance with magnetism, must know that to rub any beams of iron with powerful electro-magnets will, of course, produce very great effects upon the compasses in their neighbourhood.

Captain SELWYN: No; not unless they are polarized.

Sir FREDERICK NICOLSON: Well, I mean if they are done in a proper manner. The question is this—have all these compasses been affected alike? Can the navigator of that ship feel confident that the starboard compass, the poop compass, the port compass, are all pointing in the same direction. I have been told that there is a variation of $4\frac{1}{2}$ points between two of those compasses. [Captain FISHBOURNE: That is not correct.] I am only stating what I have been told by excellent authority. And more than that—that the magnetic force which has been produced upon those beams by the operation of rubbing, has altered considerably; as much, in about two months, as one point in one compass, so that we have here two variable elements of great importance. If we could feel that thorough confidence in the operations of Mr. Hopkins that he feels himself—if we could feel thoroughly convinced that by these operations we are to get rid of, and to get rid of permanently, the greater part of the deviation (not only get rid of it for a time, but get rid of it altogether), then there can be no question that Mr. Hopkins has done us a great service; and if those experiments are unsatisfactory to him, all I can say is that it is a great pity they were not continued. However, I have reason to know that Mr. Hopkins reported on the 24th of January that he had finished his operations. It was in consequence of that report that the operations ceased. With regard to the experiment to-night, I would only say one word. It appears to me that to use a large needle like that, which is so long in proportion to the width of the model and to the magnets along it, it cannot give proper results. I am perhaps not aware what those results are; but I cannot help thinking that that compass card is in a very different position with regard to the iron round it, from what a small needle, which you may call an infinitesimal needle compared with the length of the ship would be with regard to the iron in the hull of a ship. I only hope that if there is any doubt in this matter that those who are able to give Mr. Hopkins further facilities for carrying on his experiments will do so. The subject, as it has been truly remarked both by Captain

Fishbourne and by the lecturer, is one of vast importance. I do hope one thing —that if there is any correspondence, that it will be moved for by some Member of Parliament, in order that the public may see it. I think it would be of the greatest advantage on all sides that we should clearly know and understand, as we did the other night about turrets and broadsides, what men in an official position think about these things. I am sure when people come together, and these subjects are discussed amicably, the truth is more likely to be arrived at than when officials tie up their reports, and let no one see them.

Captain INGRAM: I think that the present Admiralty would give every facility for all these experiments.

The CHAIRMAN: The question before us is the de-magnetization of iron vessels. Upon that subject we shall be happy to hear what you have to say. We are not here to consider what the Admiralty will do or will not do.

Captain INGRAM: I can only say that I have had an iron ship. Her compasses deviated about two points. I have steered with her in a thick fog, having made every allowance, and hit our mark. Captain Strange swung us. We were swung in Portsmouth harbour and at Sheerness.

Captain FISHBOURNE: But that is not the question.

Captain INGRAM: I thought that the swinging a ship was to find out the deviation of the compass. By swinging the ship you can find the deviation of the compass, and you can allow for the error.

Mr. BELL GALLOWAY: I will make a few remarks in strict accordance with the subject of discussion. I admit that the subject is one of paramount importance. It is a question involving the value of every iron ship in Her Majesty's Navy, and in the mercantile marine of the country. The paper states that, in consequence of the "Northumberland" being built in a certain position, that position necessarily allowed the construction to absorb into it a certain magnetic power. The remedy proposed is to de-magnetize the hull, and thus reduce the ship to a normal condition. There is, however, in connection with the question, I submit, some other considerations. The first thought that strikes my mind is this, that if a ship in a certain position, in a given time, can absorb within itself a certain amount of magnetic power, the same law will affect it when at sea or in harbour. So far as an opinion may be formed upon the merits of the invention, I think generally we shall be agreed that it is quite possible to de-polarize the hull and bring it into a normal condition. But I think it will be admitted that the evil may again recur. [Captain FISHBOURNE: No.] I am only going to say, in connection with that remark, that I am exceedingly glad, and I hope that at every meeting such as this, the primary objects and the value of every plan affecting the interests of the country will specially form part of the observations that are made here. (Question.) If I am to be so frequently called to order, I really don't know how I can proceed. I hope I am dealing with the question, but these interruptions throw me out.

The CHAIRMAN: You are quite in order.

Mr. BELL GALLOWAY: I am exceedingly obliged, but gentlemen will excuse me. I am not a very educated man. I worked with George Stevenson when I was a youth, and I have had to work my way through life. I know well I speak to practical men, and I say it here boldly, that there have been various plans of paramount value and national importance submitted, from time to time, to the several public departments, and that those plans have been met with cold neglect and disrepect, to the dishonour of our nation. Having said so much, which would not have been said but for the little interruption, I come direct to the question. And the question is this, a compensating compass, constructed upon correct scientific principles, is the best in my humble opinion to support and effectively preserve the normal state of every vessel which has been operated upon by Mr. Hopkins, and I give him credit so far, and I submit that, after that, you want a perfect compensating compass, and that that compass can be produced.

Captain SELWYN, R.N.: If you will allow me, Mr. Chairman, to pass to the illustrating board, I shall perhaps, in some points, make my meaning more clear than I should otherwise be able to do. I, of course, join every other speaker in recognising the enormous importance of this subject, not less to those who follow

the sea as a profession, than to those who are carried on it; and as a very great number of soldiers will be among these, they will also recognise the importance of the subject in this Institution. Now I should wish to point out that if, as some might imagine, we were here merely to listen to a very able discourse, and to see cleverly illustrated the fact, that magnets affect magnets when they are placed near each other, we might go away, feeling that though we had been amused, we had not been taught very much. But I submit that there is a much deeper lesson to be learned. It is that such a thing as induced magnetism exists outside and apart from the natural attraction of a mass of iron. That is, the attraction of an ordinary iron ship may be often quite a different case from that of a ship which, by the operation of hammering, or the meridian in which she has been laid down, or other natural causes, has become herself a great magnet, and is then capable of inducing magnetism in all masses of iron in its vicinity, always of a contrary name. Now the very first illustration I wish to make of that is, to ask you to remember a little instrument called the astatic galvanometer, in which two needles are placed with their poles opposite. The whole object of that arrangement is to produce a needle which oscillates extremely slowly; which has no directive tendency due to the earth at all; which will constantly point east and west, instead of north and south, and which is therefore most sensitive to any electric current passing round it. Now when you take magnets and put them into a ship as correctives of erring magnets, or when you take a Barlow's plate—which, if I draw the compass bowl on the standard compass, as we generally have seen it applied, and as it is still applied, is an iron circular plate placed in that position, in front or rear of, and in close vertical proximity to the bowl—what have you done? You have, it is true, rendered the compass insensible to the magnetism outside it; but you have not less done so with regard to the magnetism of the earth, than with regard to the magnetism of the ship. You have made by your error an excessively sluggish compass, whose evils Captain Fishbourne has by no means exaggerated. So if you place, as is done in the present day in the merchant service, magnets under the deck, you have done precisely the same thing in another direction. By destroying the tendency of the magnet to point to the north pole—(I must be allowed to point out that in using these terms we are always using them wrongly; in fact, it is the south pole of the magnet that points to the north)—we have lessened the tendency of this needle, which is our "guide, philosopher, and friend" to point truly; and we have made it only sensitive to the near actions of bodies which we have interposed. Therefore I say that when the Compass Department adopted such suggestions as the Barlow plate, when they adopted such other suggestions as corrective magnets, and accepted deviations, when they refused to displace such magnets, though conscious that they sometimes only made the errors greater, then we were going in the wrong direction. Instead of eliminating the errors, we were piling up other errors in attempting the removal of those we had already got. If, on the contrary, we take the ship, and we consider her as a mass of iron whose attraction for the compass must necessarily be felt, but whose induced magnetism, which is quite a different thing, is entirely in our own power, so much so that, given any compass at any part of any ship, we can, by properly managing the induced magnetism, entirely do away with the evil effects of it, we shall be proceeding in the right direction. I cannot say that in an iron ship we can make the compass as sensitive as in a wooden ship, for this would be to presume that there is no great mass of magnetic matter or attractive matter near. That we cannot do. But we can do away, by scientific treatment, with the great evil of induced magnetism. This is what Mr. Hopkins has most ably shown us can be done. Whether he has carried it to the extreme point of perfection, which the Compass Department, long as it has existed, ought now to have attained, is quite another question. But I do say, that a body claiming to be a scientific body, as I presume the Compass Department is, should be very far from refusing credence or acceptance to those who come before it with a new plan, which does something which they could never do in their lives before, because he does not at once do the whole of that which they could never do. This is the state of the case at the present time. I do say it is one of those lamentable instances where vested interests make men blind. Unfortunately, I am afraid none of us are exempt from such blindness; but

I think this is one of those instances where the screw from the outside may properly be put on with pressure, where public opinion may say, "We care nothing whether these were methods which you were taught, and which therefore you believe; but we do care a very great deal whether one life, let alone ten or ten thousand, is or is not sacrificed to the errors of the compass, and to the consequent wrecks of ships." It is not a question which should ever be decided by the Compass Department at all; they are not the persons to do it in any sort of way. For not among those who have sedulously believed from their youth in a certain system, will you ordinarily find the adoption of new ideas. They go in the groove in which they have been taught, but they never adopt new principles. It is too much to expect of human nature. But do not let us allow them to stop everybody else. I want to see this subject taken up fairly by scientific men in a scientific spirit, not in a spirit of partisanship, but because these are great truths, which affect materially the happiness of thousands yet unborn, and because truth is to be sought, though she be at the bottom of the well. Now to follow out this illustration a little further, I beg you to observe that every ship having been made a magnet by the induction process, is herself suspended in water over another great magnet, namely, the earth; and that the familiar instances which we have constantly round us of masses of metal lying for some time in a certain direction, such as pokers, becoming by the mere inductive action of the earth, magnets themselves, are sufficient to convince us that this earth's magnetism is capable of re-acting on the ship's magnetism, and that the errors in the ship's compasses from that source are to be guarded against most closely; not alone by swinging the ship at Sheerness, to ascertain what she is going to do in South America, but by close astronomical observations, combined with a record of those observations, such as to enable us to say how much good Mr. Hopkins or any other person may have done for us, or in how much he may have failed to do it. In this Institution our object can but be, that the course of science should be advanced. I do say that this is one of those subjects which should be strongly recommended, in the face of anything the Compass Department may say, and should be followed out to what the Americans call the "bitter end."

Mr. Cornish: May I ask if the dip of the needle is correlatively affected by the deviation?

The Chairman; No. The dip of the needle is the angle which it makes with the horizon, in the plane of the declination, or deviation from the true meridian.

Captain Selwyn: There is another observation with which you will allow me to supplement what I have said before. It is that the beams which before were acted upon having been only in the neighbourhood of certain compasses, would affect them, but could not be expected to affect the whole of the compasses of the ship.

Captain Beamish, R.N.: Can you tell us whether a beam in a state of magnetism would have the same effect on a ship's compasses, as the ship's sides have?

Mr. Hopkins, jun.: A greater, but a different effect.

Captain Beamish: A different sort of effect, because the ship is built with her head to the north, and her stern to the south, while her beams run east and west? Are the compasses affected in the same sort of way as they are by the sides?*

---

* The following answer, omitted at the time, was subsequently communicated.—Ed.

"The magnetism of the transverse beams in vessels built in a N. & S. direction, has been carefully examined by my father, in the case of the "Northumberland." These iron beams which had acquired polarity while in an east and west position—the ship being north and south in building—were found to have, not two dissimilar poles at their extremities, like an ordinary magnet, or like the hull of the ship itself, but *a north pole at each end, and a powerful south pole in the centre.* Immediately over this central south pole of the beam, the poop binnacle of the "Northumberland" has been placed.

This singular feature in the magnetism of the transverse beams has been entered into in Mr. Hopkins's report to the Admiralty, and the nature of these magnetic currents have been minutely delineated in the diagrams accompanying that report."—E. H.

Sir FREDERICK NICOLSON: I should like to ask you, did you apply the electro-magnets to any of the iron stanchions in or near any of the other compasses?

Mr. HOPKINS, jun.: Not near the compasses.

Sir FREDERICK NICOLSON: Where? perhaps you will tell us in your reply.

Mr. HOPKINS, jun.: I must again throw myself on your kind consideration. I assure you that if my father had been here, he would have been able to answer every question satisfactorily. The first point that has been referred to by Admiral Ryder was whether the process would remove the heeling error. My father maintains that it would; that the whole of the magnetism being taken away from the beam, there would be nothing to affect the compass. Mr. Grover asked the question as to whether it was easy of application. With the assistance we had, we only had four riggers—no other people helped us, and I carried my father's instruments on board—we found it very easy indeed, far easier than I at first apprehended. Sir Frederick Nicolson first of all alluded to the fact that Captain Evans had maintained after my father's first operations on the "Northumberland" that no perceptible change had taken place on the compass. But Captain Evans referred to a compass situated on the poop or in the middle line of the ship. My father was aware of the fact that at that place there was no effect; but inasmuch as the compasses would be exposed to the magnetism of the beams, what my father proposed to do on that first occasion, was to remove the magnetism from the hull; and that compass held within a few feet of the hull on the exterior all round, showed a considerable reduction of the acquired magnetism. With reference to another point to which Sir Frederick Nicolson alluded, viz., what compasses were referred to on board the "Northumberland," as having their deviation reduced:—The poop compass is placed about four feet from an iron-beam; the two steering compasses stand 3 ft. 8 in. underneath the same iron-beam. Of course, it is clear that these steering compasses are too near the iron-beam, whether it is in a neutral state, or whether it is in a magnetic state. But the effect upon the poop compass is recorded in the table which I have already handed in. A corresponding improvement was produced upon the two steering compasses, but not to the same extent, standing as they do, within so short a distance of the beam. From the fact of the beam having been covered with wood, the beam could not be got at.

Captain FISHBOURNE: One-half only.

Admiral RYDER: Could not the wood have been stripped off?

Captain FISHBOURNE: Yes, but they had no authority.

Admiral RYDER: Captain Cochrane had none?

Captain FISHBOURNE: He had nothing to do with it.

Mr. HOPKINS, jun.: Then, it was objected that the standard compass remained exactly in the same state. The standard compass stands about there (pointing); and it was asked, how was it, while there was an improvement upon the poop and the two steering compasses, that the standard compass showed no change at all? That may be answered very simply. The beams underneath the standard compass were left alone, and not touched. So this compass just illustrated the fact, that whilst the hull was de-polarized, no perceptible change took place in the compass unless the beams were de-polarized also. Here four or five beams were de-polarized; this last beam was not completed. A question has also been raised as to whether the effect will last. I think my father met this point. I have here a note with regard to the possibility of the ship, after undergoing de-magnetization, becoming magnetic a second time. He says it has been proved that such an effect never occurs. The hard iron with which a ship is constructed will retain its acquired magnetism for many years, but when it is once destroyed, it will not return, unless the ship be exposed to re-hammering. I do not think there is any other point that I need now dwell upon. I may state that my father offered that some officer should be sent on board, but it was not responded to.

Sir FREDERICK NICOLSON: In the table you showed us a little while ago, there is no allusion to the starboard compass. Is there any reason for that? There is only allusion to the poop compass.

Mr. HOPKINS, jun.: The reason of that is this:—Before commencing any operation at all, the ship had been swung, and the table of deviations was handed to us.

That table did not record the deviations of the starboard compass, but only of the port compass, and the consequence was we took that column.

Captain FISHBOURNE: I may mention that the reason why the deviations of the starboard compasses were not given was this, that the indications were so unsatisfactory that they would not record them. They had to shake the compasses to get indications. That shows the advantage of such a table.

The CHAIRMAN: I am sure the meeting will return their thanks to Mr. Hopkins for the very interesting and valuable paper which he has read on behalf of his father, whose absence I, in common with all present, deeply regret. I wish to make one or two observations with regard to the operations which have been performed, not with the view of criticising unfavourably the result we have heard this evening, because I think we must all be satisfied that, as far as the experiments have been conducted, they have been complete, and have been satisfactory also; but with regard to the de-magnetization of the beams, I think it is only fair to Mr. Hopkins to observe, that if in the operation of passing the magnets, they are not carried beyond what may be called the poles of the magnet beam, they produce secondary poles; and, therefore, in these beams, which were only partially stripped, and where the electro-magnets could only be brought to bear upon a certain portion of their length, they could not have produced the same effect which they would have produced if carried beyond their poles. Therefore, if there is any incompleteness in that, it is capable of explanation. Then, with regard to the de-magnetization of the skin of the ship, as far as the experiment this evening has been shown, it must be satisfactory to all present that the effect on the model was complete both ways: first, when the magnetism was induced in the skin of the model; and, secondly, when it was driven out of it by the contrary process. But I do not think the experiment, as shown here, quite represents that which would have been conducted upon a ship, inasmuch as I believe that, independently of the skin which we have here, a ship is constructed upon ribs, that is to say, upon bars of iron, always vertical, or nearly always in a vertical position, but which, under the earth's inducing influence, may become magnetized. I cannot believe that the process which has been performed here upon this iron plate would have the same effect upon an iron ship, plated not only with iron outside, but also with vertical ribs, those vertical ribs being induced magnets. I mention this because in an experiment of this kind we cannot altogether connect the results with the experiments upon the "Northumberland;" but I have no doubt that the results must have been, to a very large extent, successful, or the action of the ship's magnetism upon the compass would not have been reduced from 53 to 17 degrees. I believe there may be in the construction of a ship, that is to say, in the existence of its ribs, a means of return to a state of induced magnetism, which would not exist in a model of this kind. Then, with regard to what a gentleman behind me observed, that in swinging ships you acquire a knowledge of deviation, that is quite true. I merely notice the fact as an apology to anybody whom I may have called to order for making observations not strictly in accordance with the paper. The swinging of a ship is merely a method of determining the action of the ship upon the compass; it has nothing to do with the magnetism of the ship, considered abstractedly. My friend Captain Selwyn has made a highly instructive set of observations upon this subject. There is one point to which he referred, and which I believe we must all consider as the one which a person thrown upon his own resources would naturally adopt in determining what the action of the ship is upon the compass; that is, by astronomical observations. There is one point—it is this, that in any geographical position, to determine what the action of the ship is upon the compass, it will not be sufficient to take the astronomical observation with the ship's head in one direction, to know what the effect of the ship is upon the compass in any particular geographical position; the observation must be repeated with the ship's head in eight, or ten, or whatever number of different positions it may be considered desirable to do so. So it will not suffice to consider the action of the ship upon the compass in one given direction, but it must be taken in a great many directions. I mention this to show that the subject is not quite so simple as it might appear to be. [Captain SELWYN: That is our practice.] I believe Mr. Hopkins wishes to offer some observations before I offer the thanks of the meeting to him.

Mr. HOPKINS, jun.: There is just one word I wish to say. I should be happy to substitute a smaller compass here, as Sir Frederick Nicolson objected to the size of the needle. A smaller compass would show it rather more accurately from being farther away from the sides, and also from the ribs. With reference to the vertical ribs, the plates are riveted to the ribs, and they form part and parcel of the whole; and as the whole is being de-magnetized, they are affected in the same way. Their magnetism does not extend towards the needle or the centre line to a greater extent than that of the hull itself.

The CHAIRMAN: I will now, with your permission, offer our best thanks to Mr. Hopkins for the paper which his father has been kind enough to send us. The subject is one, as we all admit, of the highest importance, both socially and nationally; and the country owes to all persons who come forward, as Mr. Hopkins has done, giving their time and talents to the development of a question of this kind, their deep and grateful acknowledgments, apart from all considerations of personality, all considerations of official position, and all considerations which may weigh in men's minds, except that one of striving to make navigation as safe as it can be made. This question will have further development from the gentleman from whom we have had this excellent paper to-night, and ere long we shall arrive at results which, if not conclusive, may, it is hoped, be nearly so.

Collection du Mémorial de l'Officier du Génie, 17 vols. 8vo. 5*l.*

Charras. Histoire de la Guerre de 1813 en Allemagne. 8vo., with maps. 10*s.*

Blois. De la Fortification en présence de l'artillerie nouvelle. 2 vols. 8vo. 15*s.*

Puiseux. Siége et Prise de Rouen par les Anglais, 1418-19. 8vo. 7*s.* 6*d.*

L'Armée Française en 1867 (par le Général Trochu). 8vo. 5*s.*

Batjin. Histoire de l'Empereur Napoléon I., surnommé le Grand. 2 vols., royal 8vo., with 6 plates, cloth. 1*l.* 5*s.*

Hillebrand. La Prusse contemporaine et ses institutions. 8vo. 3*s.* 6*d.*

Witzleben. Heerwesen und Infanteriedienst der Preussischen Armee. 8vo. 10*s.* 6*d.*

Rüstow. Der Krieg von 1866 in Deutschland und Italien militärisch beschrieben. 8vo., with maps. 10*s.* 6*d.*

Institutions (Les) militaires de la France. Louvois—Carnot—St. Cyr (par H. O. Duc d'Aumale). 8vo. 6*s.*

Piron. Essai sur la Défense des Eaux et sur la construction des barrages. 8vo., with plates. 7*s.*

Reymann. Topographical Special Map of Central Europe in 423 sheets, 324 of which are now published. Scale 1·200000. Subscription price for the whole map, 1*s.* 2*d.* per sheet. Prospectus to be had on application.

Ploennies. Neue Hinterladungsgewehre nach offiziellen Versuchen beurtheilt. 2 parts, 8vo. 6*s.* 6*d.*

Brettes (Martin de). Etudes sur les mouvements et les propriétés mécaniques des projectiles oblongs. 8vo. 4*s.*

Instruction élémentaire sur les Reconnaissances militaires à l'usage des officiers. 8vo. 5*s.*

Landolt. Dictionnaire polyglotte des termes techniques militaires Néerlandais, Français, Allemand et Anglais. 2 parts, 8vo. 17*s.* 6*d.*

Lelouterel. Manuel des Reconnaissances militaires. Cinquième édition. 8vo. 5*s.*

Organisation et Composition de l'Artillerie Italienne, d'après les Documents officiels. 8vo. 3*s.*

Gouvion Saint-Cyr. Journal des Opérations de l'armée en Catalogne en 1808 et 1809. Deuxième édition. 8vo. with plates. 1*l.* 5*s.*

Instruktion zum Reit-Unterricht für die K. Preussische Kavallerie. 8vo. 1*s.* 6*d.*

Königl. Preussisches Mineur Exerzir-und Dienst-Reglement. 8vo. 5s.

Königl. Preussisches Pontonir Exerzir-und Dienst-Reglement. 16mo. 2*s.* 6*d.*

Richter. Das Militär Medicinal Wesen Preussens. 8vo. 7*s.*